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FINDING THE WAY AT SEA.

BY RICHARD A. PROCTOR.

(*Continued from p.* 177.)

EVERYONE who makes a long sea-voyage must have noted the importance attached to noon observations; and many are misled into the supposition that these observations are directly intended for the determination of the longitude (or, which is the same thing in effect, for determining true ship-time). This, however, is a mistake. The latitude can be determined at noon, as we have seen. A rough approximation to the local time can be obtained, and is commonly obtained, by noting when the sun begins to dip after reaching the highest part of his course above the horizon. But this is necessarily *only* a rough approximation, and quite unsuited for determining the ship's longitude. For the sun's elevation changes very slowly at noon, and no dip can be certainly recognised even from *terra firma*, far less from a ship, within a few minutes of true noon. A determination of time effected in this way, serves very well for the ship's "watches," and accordingly when the sun, so observed, begins to dip, they strike "eight bells" and "make it noon." But it would be a serious matter for the crew if that was made the noon for working the ship's place; for an error of many miles would be inevitable.

The following passage from "Foul Play," illustrates the way in which mistakes have arisen on this point. The hero, who being a clergyman and a university man is of course a master of every branch of science, is about to distinguish himself before the heroine by working out the position of the ship *Proserpine*, whose captain is senselessly drunk. After ten days' murky weather "the sky suddenly cleared, and a rare opportunity occurred to take an observation. Hazel suggested to Wylie, the mate, the propriety of taking advantage of the moment, as the fog bank out of which they had just emerged would soon envelop them again, and they had not more than an hour or so of such observation available. The man gave a shuffling answer. So Hazel sought the captain in his cabin. He found him in bed. He was dead drunk. On a shelf lay the instruments. These Hazel took and then looked round for the chronometers. They were safely locked in their cases. He carried the instruments on deck, together with a book of tables, and quietly began to make preparations, at which Wylie, arresting his walk, gazed with utter astonishment" (as well he might).

"'Now, Mr. Wylie, I want the key of the chronometer cases.'

"'Here is a chronometer, Mr. Hazel,' said Helen, very innocently, 'if that is all you want.'

"Hazel smiled, and explained that a ship's clock is made to keep the most exact time; that he did not require the time of the spot where they were, but Greenwich time. He took the watch, however. It was a large one for a lady to carry, but it was one of Frodsham's masterpieces.

"'Why, Miss Rolleston,' said he, 'this watch must be two hours slow. It marks ten o'clock; it is now nearly midday. Ah, I see,' he added with a smile, 'you have wound it regularly every day, but you have forgotten to set it daily. Indeed, you may be right; it would be a useless trouble, since we change our longitude hourly. Well, let us suppose that this watch shows the exact time at Sydney, as I presume it does: I can work the ship's reckoning from that meridian, instead of that of Greenwich.' And he set about doing it. Wylie, after some angry words with Hazel, brings the chronometers and the charts. Hazel 'verified Miss Rolleston's chronometer, and allowing for difference of time, found it to be accurate. He returned it to her, and proceeded to work on the chart. The men looked on; so did Wylie. After a few moments, Hazel read as follows: West longitude 146° 53′ 18″, South latitude 35° 24′. The island of Opara* and the Four Crowns distant 420 miles on the N.N.E.' and so on. And, of course, 'Miss Rolleston fixed her large soft eyes on the young clergyman with the undisguised admiration a woman is apt to feel for what she does not understand."

The scene here described corresponds pretty closely, I have little doubt, with one actually witnessed by the novelist, except only that the captain or chief officer made the observations, and that either there had not been ten days' murky weather or else that in the forenoon, several hours at least before noon, an observation of the sun had been made. The noon observation would give the latitude, and combined with a forenoon observation, would give the longitude; but *alone* would be practically useless for that purpose. It is curious that the novelist sets the longitude as assigned much more closely than the latitude, and the value given would imply that the ship's time was known within less than a second. This would in any case be impracticable; but from noon observations the time could not be learned within a minute at the least. The real fact is, that to determine true time, the seaman selects, not noon, as is commonly supposed, but a time when the sun is nearly due east, or due west. For then the sun's elevation changes most rapidly, and so gives the surest means of determining the time. The reader can easily see the *rationale* of this, by considering the case of an ordinary clock-hand. Suppose our only means of telling the time was by noting how high the end of the minute-hand was; then clearly we should be apt to make a greater

* The island fixes the longitude at about 147°, otherwise I should have thought the 4 was a misprint for 7. In longitude 177° west, Sydney time would be about 2 hours slow, but about 4 hours slow in longitude 147° west.

mistake in estimating the time when the hand was near XII. than at any other time, because then its end changes very slowly in height, and a minute more or less makes very little difference. On the contrary, when the hand was near III. and IX., we could in a very few seconds note any change in the height of its extremity. In one case we could not tell the time within a minute or two; in the other we could tell it within a few seconds.

But the noon observation would be wanted to complete the determination of the longitude; for until the latitude was known, the captain could not be aware what apparent path the sun was describing in the heavens, and, therefore, would not know the time corresponding to any particular solar observation. So that a passenger, curious in watching the captain's work, would be apt to infer that the noon observations gave the longitude, since he would perceive that from them the captain worked out both the longitude and the latitude.

It is curious that another and critical portion of the same entertaining novel, is affected by the mistakes of the novelist on this subject. After the scuttling of the *Proserpine*, and other events, Hazel and Miss Rolleston are alone on an island in the Pacific. Hazel seeks to determine their position, as one step towards escape. Now, "you must know that Hazel, as he lay on his back in the boat, had often in a half-drowsy way, watched the effect of the sun upon the boat's mast: it now stood, a bare pole, and at certain hours acted like the needle of a dial, by casting a shadow on the sands. Above all, he could see pretty well, by means of this pole and its shadow, when the sun attained its greatest elevation. He now asked Miss Rolleston to assist him in making this observation exactly. She obeyed his instructions, and the moment the shadow reached its highest angle and showed the minutest symptom of declension,* she said 'Now,' and Hazel called out in a loud voice" (a soft voice would have served as well) "'Noon!' 'And forty-nine minutes past eight at Sydney,' said Helen, holding out her chronometer; for she had been sharp enough to get it ready of her own accord. Hazel looked at her and at the watch with amazement and incredulity. 'What?' said he, 'Impossible. You can't have kept Sydney time all this while.' 'And pray why not?' said Helen. 'Have you forgotten that some one praised me for keeping Sydney time? it helped you, somehow or other, to know where we were.'" After some discussion, in which she shows how natural it was that she should have wound up her watch every night, even when "neither of them expected to see the morning," she asks to be praised. "'Praised!' cried Hazel, excitedly, 'worshipped, you mean. Why, we have got the longitude by means of your chronometer. It is wonderful! It is providential. It is the finger of Heaven. Pen and ink, and let me work it out.'" He was "soon busy calculating the longitude of Godsend Island." What follows is even more curiously erroneous. "'There,' said he. 'Now the latitude I must guess at by certain combinations. In the first place, the slight variation in the length of the days. Then I must try and make a rough calculation of the sun's parallax.'" (It would have been equally to the purpose to have calculated how many cows' tails would reach to the moon.) "'And then my botany will help me a little; spices furnish a clue; there are one or two that will not grow outside the tropic,'" and so on. He finally sets the latitude between the 26th and 33rd parallels, a range of nearly 500 miles. The longitude, however, which is much more closely assigned, is wrong altogether, being set at 103½ degrees *west*, as the rest of the story requires. For Godsend Island is within not many days' sail of Valparaiso. The mistake has probably arisen from setting Sydney in *west* longitude instead of *east* longitude, 151° 14′; for the difference of time, 3h. 11m., corresponds within a minute to the difference of longitude between 151° 14′ west and 103½° west. The whole account is crowded with mistakes.

Mere mistakes of calculation, however, matter little in such cases. They do not affect the interest of a story even in such extreme cases as in "Ivanhoe," where a full century is dropped (in such sort that one of Richard the First's knights holds converse with a contemporary of the Conqueror, who, if my memory deceives me not, was Cœur de-Lion's great-great-grandfather). It is a pity, however, that a novelist, or indeed any writer, should attempt to sketch scientific *methods* with which he is not familiar. No discredit can attach to any person, not an astronomer, who does not understand the astronomical processes for determining latitude and longitude, any more than to one who, not being a lawyer, is unfamiliar with the rules of Conveyancing. But when an attempt is made by a writer of fiction to give an exact description of any technical matter, it is as well to secure correctness by submitting the description to some friend acquainted with the principles of the subject. For, singularly enough, people pay much more attention to these descriptions when met with in novels than when given in textbooks of science, and they thus come to remember thoroughly well precisely what they ought to forget. I think, for instance, that it may not improbably have been some recollection of "Foul Play" which led Mr. Lockyer to make the surprising statement that longitude is determined at sea by comparing chronometer time with local time, which is found "at noon by observing, with the aid of a sextant, when the sun is at the highest point of its path." Our novelists really must not lead the student of astronomy astray in this manner.

(To be continued.)

* This would be a most difficult matter to determine: Hazel should have taken the task himself (even he would have made nothing out of it, though Mr. Reade makes so little of it): he might safely have left Miss Rolleston to call out "Noon" for him when he had noted it.

THE PHILOSOPHY OF CLOTHING.

By W. Mattieu Williams.

XVI.—BOOTS AND SHOES.

I NOW approach a very difficult part of my subject; the philosophy of foot clothing. The question of whether we should clothe the feet at all, even in these and higher latitudes is fairly debateable. Our ordinary devices for imprisoning, strangling, cramping, and suffocating them are demonstrably abominable, but how we are to proceed in carrying out the much-needed boot and shoe reform is not easily decided.

Let us examine the structure and functions of the foot, and then ask ourselves whether we are obeying or disobeying the injunctions which these express, whether our boots and shoes—especially boots—are assisting or hindering the performance of the natural functions of the feet.

Beginning with the skeleton of the human foot. The first fact that presents itself is that it is built up of a

number of small bones—no less than twenty-six. Even the most solid part of the foot, from the heel to the instep, that upon which the ankle rests, is in seven pieces, and there are five more in front of these before the toe bones are reached. These twelve bones form an arch upon which the whole of the body rests, and this arch is specially constructed to combine firmness with elastric spring, and in the forward or toe-part of the foot the fourteen bones, with their muscles, tendons, and ligaments, we find arrangements for considerable flexibility. None of the bones of the foot directly touch each other as bones; the junctions of all are lined with cartilaginous matter, preventing vibratory jar.

A very important function, but little understood, of this complexity is to enable the foot to co-operate with the other buffers of the body in protecting the brain from concussion. In running and jumping, a mass weighing from 150 to 250 lb. is repeatedly falling to the ground, and striking it with considerable violence. If it were a rigid mass, a vibratory shock would travel through it up to the skull at each collision with the ground, and such a shock would seriously damage the brain, arresting, or more or less completely stopping, the mental functions. But we are protected against such vibratory shock—first, by the structure of the skull, which is made up of several pieces, and thus cannot vibrate as a whole; by the thick, elastic cartilages that lie between each of the twenty-four pieces of the spine; then by the cartilages of the hip and thigh-bones; by other cartilages in the knee-joints; and finally, by those of the ankle and the complex mechanism of the foot already described. As the maximum weight rests on the foot, its elastic response is primary and important.

It is evident that a tight boot made of material yielding so little as thick leather, must cripple and frustrate the functions of this beautiful and beneficent mechanism. This is shown by the tottering steps of a tightly-booted human being. The tottering step of old age is largely due to the decay, the withering or drying up of and consolidating of the springs above described; but a young man or woman, or even a boy or girl, may be, in this respect, brought prematurely to a condition of old age by tight boots. This description applies not only to the torture-chambers of imbeciles who intentionally squeeze their feet in order to make them appear unnaturally small, but to ordinary average boots, such as an average boot-maker supplies when they are "made to measure." In order to obtain a respectably fitting boot or shoe I have found it necessary to wear two or three pairs of thick woollen socks one over the other when going to be measured. A conventional fit for this enlarged foot becomes a decent fit for the actual foot.

According to the natural structure of human toes, they should spread out with space between each other, forming the widest part of the foot when it is planted on the ground. They should also, by virtue of the muscles attached to them, exert some prehensile power in climbing, and in the final projecting effort of running. The victims of ordinary boots inherit degenerate feet, in which these functions are practically annulled, and before they attain middle age the large toe is actually turned inwards, and all the toes squeezed together. The naked foot of a full-grown fine lady is usually a very disgusting object; that of an infant or a Highland lassie is very beautiful.

The supply of sweat glands is—area for area—more abundant on the hands than on any other part of the body, and next to the hands, the feet are the best supplied. The lesson we should learn from this is obvious enough. Both hands and feet should be specially free to perform their natural functions of superabundant exhalation. The wearing of leather gloves, merely for appearance sake, should be denounced as contemptible foppery. Protection of the hands from cold, from dirt in exceptional cases, or from mechanical injury, as in trimming thorn hedges, &c., is, of course, a full justification of the practice; but the housemaids' gloves, gardeners' gloves, driving gloves, &c., are very different from the finger-pinching hand-prisons of the fop. I have heard of women who have reduced themselves to such a condition that they are liable to take cold if their hands are wetted by water of ordinary temperature. Poor things!

But what shall we say of the condition of ordinary average conventional feet? What a satire upon our ordinary practice of foot-clothing is the almost universal dread of wet feet! The human foot, with its special sole integument, is specially constructed for exposure to cold and wet ground: and, when it has not been artificially injured by false imprisonment, no inconvenience follows its free exposure to rain-sodden or snow-covered ground during the course of ordinary locomotion. Young ladies in English boarding-schools are martyrs to chilblains. Highland girls, who walk bare-footed to school through frost and snow, and rain and sleet, rarely know what such things mean, and the slight acquaintance they have is of recent origin, since their partial adoption of southern foot-gear.

I have no hesitation in positively affirming that whoever is liable to take cold by temporarily wetting the feet has diseased feet. This form of disease very frequently reaches the stage of putrescence of the perspiration of the feet. We hear of many remedies for this, and of their failures, but there is one that will not fail if persistently applied, that is, continuing bare-footed and usually wet-footed. I know that it is not easy—in most cases practically impossible—to apply this fully, and caution should, of course, be used at first, but if every available opportunity were used of absolutely uncovering the feet, and at all other times wearing the most loose and porous shoes or slippers, with soft, absorbent woollen socks, great relief would be afforded.

If there was no such things as tin-tacks, pins and needles, broken glass and crockery, I should be disposed to advocate the total abolition of boots, shoes and stockings, and the furnishing of all street-doors with water-troughs and towel-mats for the use of entering inmates and visitors. Natural asperities of ground (barring thorns), are easily overcome by the remarkable facility with which the sole of the foot thickens when exposed to mechanical irritation. It is a fact not generally observed that this hard integumentary structure extends not only over the bottom of the foot, but to a considerable extent up its sides, especially at the heel. I regard corns as plaintive protests made by tortured feet against the ill-usage they receive. A corn is a local demonstration of the natural thickening capabilities of the skin of the foot on the application of suitable stimulants. If the whole of the working surface were properly exposed, such callosity would extend throughout, and would be protective instead of painful. The corn is a thorn in the flesh, a local spear-like hardening, that penetrates the surrounding tenderness instead of covering and protecting it.

My first experience in barefoot walking dates as far back as 1841. In the autumn of that year I was at the Athol Gathering, then a genuine Highland festival of the peasantry (subsequently a fashionable aristocratic assembly). On the morning after the ball at the Brig of Tilt, where I was the only trousered Sassenach, I started

to walk to Braemar through Glen Tilt, and was overtaken by a Highland lassie, a beautiful specimen of a noble race. She had been the belle of the ball, and was engaged to its hero, a stalwart Cameron, who had carried off all the best prizes at the athletic competition. We walked on together, but though an able pedestrian, I had a struggle to keep pace with my vigorous barefooted companion. Observing this, she suggested that I should cast off my shoon and be free. I did so, and the ground being favourable for a beginner, perceived at once that my natural pace became the same as hers without the forcing or strain that was previously necessary. In the course of many thousands of miles of subsequent pedestrian excursions I have frequently done the like with similar advantage, though unable to continue a long distance on account of the tenderness of artificially-swaddled soles. Crossing a sandy bay at low tide is very tempting, and if the sands are hard a curious result follows a long walk. After a few miles a slight tenderness is felt in the ball of the large toe. This increases, and presently becomes painful. On examination, the cuticle of that part is found to be ground smoothly away by the friction on the sand. My first experience of this was in crossing one of the fine bays near the Giant's Causeway. Unconscious of what was going on I persevered until the pain grew serious, and then found that the cutis was bared. The next day I was unable to walk at all. I name this as a warning to others who may be induced to try experiments in barefoot walking.

When bare feet are objectionably conspicuous, or the ground severe, I walk on for an hour or two in orthodox fashion, then stand ankle deep in a brook; or pour water in my shoes, and keep my feet scrupulously wet during the rest of the day. This adds about 20 per cent. to the possible mileage. One consequence of this, and the habit of usually wearing very loose slippers, is that, though no longer a boy, I laugh at the idea of suffering any evil consequences from wet feet, whether walking, standing, or sitting.

OPTICAL RECREATIONS.

By "A Fellow of the Royal Astronomical Society."

(*Continued from Page* 541 *of Vol. VII.*)

"IF," says Sir David Brewster in his *Optics*, "We transmit a beam of the sun's light through a circular aperture into a dark room, and if we reflect it from any crystallised or uncrystallised body, or transmit it through a thin plate of either of them, it will be reflected and transmitted in the very same manner, and with the same intensity, whether the surface of the body is held above or below the beam, or on the right side or left, or on any other side of it, provided that in all these cases it falls upon the surface in the same manner; or, what amounts to the same thing, the beam of solar light has the same properties on all its sides; and this is true, whether it is white light or directly emitted from the sun, or whether it is red light, or light of any other colour. The same property belongs to light emitted from a candle or self-luminous body, and all such light is called *common* light."

Now we have previously seen in the course of these Essays (Vol. V., p. 352, *et alibi*) that light consists of a series of undulations or vibrations transverse to the direction in which it is propagated, and it is pretty evident that these vibrations must occur symmetrically as regards the axis of the beam. Let us suppose that in Fig. 44 the left-hand circle represents a section of a beam of light coming through a small round hole into a darkened room, then will the vibrations of which it is

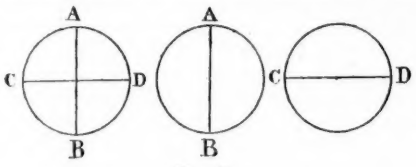


Fig. 44.

made up be performed in the direction C D, as well as in that A B. Or possibly an extremely simple model, which the reader may make at once, will render our meaning a little clearer. Let him, then, get a piece of card, and with a penknife cut out two undulating strips like V^1 (Fig. 45). A horizontal slit in one of them will

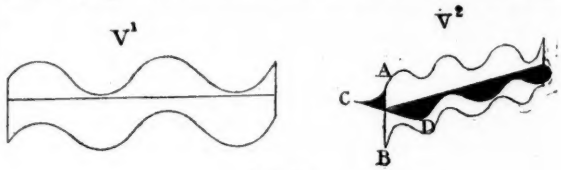


Fig. 45.

enable the other to be inserted through it at right angles, and we shall obtain the V^2 of the same figure, which will represent our symmetrical beam of light—symmetrical because it is obviously alike in every direction. Is it possible in any way to alter this disposition of the vibrations of the ether, and so obtain a beam with *sides?* Let us see. Many of our London readers must recollect the shop of the late Professor Tennant in the Strand, and the splendid rhomb of Iceland spar which used to be exhibited in the window, showing a double image of a red wafer placed behind it. A description of this will form an introduction to the study of the so-called "polarisation of light," to which we are now about to address ourselves. A rhomb, as most people are aware, is a solid bounded by six equal and similar rhomboids—a rhomboid being defined by Euclid (Definition XXXIII.) as having "its opposite sides equal to each other; but all its sides are not equal, nor its angles right angles." Well, whether we find Iceland spar in a crystalline or massive condition it will always split up into rhombs, like Fig. 46.

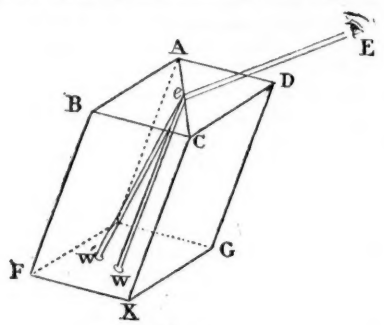


Fig. 46.

The opposite sides of a crystal of this sort are parallel, and the line AX (known as the axis of the crystal) is equally inclined to all its six faces at an angle of 45° 23′. Suppose now that we obtain such a crystal (the larger it

is the better, and at all events the length of one of its edges must be at least an inch), and making a circular dot, W, on a sheet of paper, place one face of the crystal on it. Then, to an eye placed at E, the dot will be seen double, as W, W^1, and if we turn the crystal round, the same side always touching the paper, we shall see the dot W remain apparently stationary, and the dot W^1 describe a seeming orbit round it. Now we have several times insisted, in these papers, on the fact that light goes and returns by the same route, and hence the reader will be prepared to expect that if instead of viewing a spot from E we there place a source of light, the beam E*e* will be split on entering the crystal, and its image be seen as two to an eye placed on the other side of what was the base of our crystal in our first experiment. Moreover, when we come to investigate the bending of this double ray, we shall find that while *e*W follows the ordinary law of sines (vol. vi., p. 32), and lies, of course, in the plane of incidence, eW^1 does nothing at all of the sort, but follows some new and remarkable law. Hence *e*W is called the ordinary ray, and eW^1 the extraordinary ray. It may not, perhaps, be wholly out of place to add here that when the ray E*e* is incident perpendicularly on the face A B C D, as in other cases, the *ordinary ray* passes straight through the crystal, and is not refracted at all, while the extraordinary ray, under these circumstances, has an angle of refraction of 6° 12′, and is bent on one side. Lastly, we may note that a ray of light passing in the direction A X suffers no double refraction whatever.

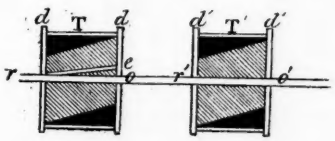


Fig. 47.

Now we have gone into all this seemingly irrelevant detail, because in splitting the incident beam of light into two our crystal has done something else: in point of fact it has separated our original beam A B C D (Fig. 44) into two, A B and C D, each having different properties on different sides; or rather, A B has the same properties at its sides A and B that C D has at its sides C and D. In other words, the vibrations of which the beam of common light A B C D is made up have been split into two separate sets A B and C D at right angles to each other. Each, then, of these separate beams is said to be polarised, and the planes passing through the lines A B and C D are called the planes of polarisation of the beams respectively. From this we may infer, what we shall find experimentally to be the fact, that a beam of common light A B C D may be regarded as made up of two beams of polarised light with their planes of polarisation at right angles to each other; and that if we superposed these polarised beams so that their planes of polarisation, instead of being at right angles, were made coincident, we should get a beam of polarised light twice as luminous as either of the separate beams composing it. We have only to employ a second rhomb of Iceland spar to show this.

For this purpose it is better to mount our two rhombs of spar, as shown in Fig. 47, where T and T′ show two pieces of brass tube in which our rhombs are fastened by means of cork rings. Both ends of the tubes are covered by brass discs *d d d′ d′*, perforated centrally with holes. Placing our tubes, then, in a horizontal position, and letting a beam of parallel solar rays *r o* pass through the left hand opening, it will be seen, from what has preceded, that only the ordinary ray *r o* will emerge through the opposite hole, the extraordinary ray *r e* being refracted up to *e*, whence, of course, its egress is prevented by the solid brass plate. If now the two rhombs are so situated that their principal planes passing through *r o* and the optic axis lie in the same plane (say that of the paper), the ordinarily refracted ray *r o* will pass similarly through the second crystal, as at *r′ o′*. If, though, keeping the first tube fixed, we rotate the second one about its axis, we shall find that double refraction will take place, and that an extraordinary as well as an ordinarily refracted spot of light will become visible. This extraordinarily refracted ray, at first dim, becomes brighter as the crystal is rotated, the ordinary ray diminishing correspondingly in illumination until the angle between the two principal planes of cleavage of the rhombs = 45°. As the rotation continues, the ordinary ray continues to fade out and the extraordinary ray to become stronger until, when the principal planes are square to each other, the ordinary ray vanishes, and the extraordinary one remains of the full strength of the original beam *o r*. Light may also conveniently be polarised by the aid of slices of the mineral tourmaline, cut parallel to the optic axis of the crystal. Tourmaline of moderate thickness entirely extinguishes the ordinary ray, but transmits the extraordinary ray (which vibrates parallel to its crystalline axis) perfectly. Tourmaline is of all sorts of colours, some splendid specimens from Devonshire being of so dark a brown as to appear almost black. If slices of sufficient thinness can be cut from

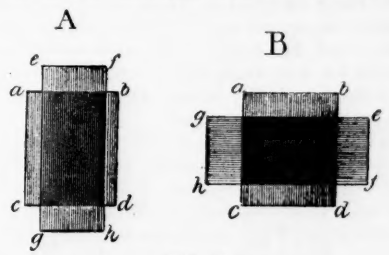


Fig. 48.

these dark crystals, they are generally effective, but this is no easy matter. Those usually found in commerce are brown, green or red. They, of course, colour the light to a certain extent if they are of a dark tint or cut into thick slices, but this is no great practical disadvantage. Two such plates mounted in cells which can be rotated, the cells being borne at the ends of a twisted wire, are sold by opticians under the name of "Tourmaline tongs." Any object required to be viewed by polarised light may be nipped between the wire rings in which the cells rotate. If now we place our two plates in such a position that their axes are parallel, as at A (Fig. 48). The light from the sun or any other source will pass through them just as it would through a single plate of the thickness of the two combined. If, though, we rotate one of the plates, as at B, the transmitted light will get weaker and weaker until, at length, when their axes are at right angles, it will disappear wholly, and darkness will supervene. And this seems a favourable point at which to pause in our description of Polarising apparatus, and to utilise the experiment just described in an endeavour to make the theory of polarisation apprehensible. To this end we may regard the internal structure of one of our plates as being that of a *grating*, with its parallel

openings vertical. If now we imagine our model of a beam of ordinary light, V², Fig. 45, to be pushed ("end on," as sailors say) against such a grating it is abundantly evident that, while the vertical series of vibrations, A B, would pass through perfectly, the transverse ones, C D, would be most effectually stopped: just as a walking-stick, held parallel to some iron railings, could be passed easily between them, but would be arrested at once if held across the direction of their length. At A, in Fig. 48, the, so to speak, cross vibrations C D, Figs. 44 and 45, have been shut out by the first plate, *a b c d*, and only the vertical ones, A B (same figs.) allowed to pass; and as our hypothetical "grating" is also vertical in the second plate, *e f g h*, the vertical undulations will get through that perfectly. But (B, Fig. 48) as the vibrations which pass through *a b c d* are all vertical, and vertical only, when we make our second supposititious grating, *e f g h*, horizontal, of course we interpose an insuperable barrier to their passage, and darkness is the result.

(To be continued.)

THE YOUNG ELECTRICIAN.

By W. Slingo.

(Continued from p. 158.)

ELECTROSCOPES.

Pr. 6.—Electroscopes are instruments for detecting the presence of a charge of static electricity, and for determining its quality. They are of a simple nature, and exhibit important and interesting phenomena, but they must not be confounded with electrometers or instruments for measuring the strength or quantity of a charge. Induction is the ruling principle in the construction and action of electroscopes.

Ex. CIV.—Anything which will indicate the presence of electricity (such as a suspended feather, pith-ball, &c.) is in a measure an electroscope; but we will confine ourselves to such instruments as respond to the more complete definition. Fig. 58 illustrates a typical form of instrument. It consists of a bottomless gas-receiver or glass-jar, F, in the neck of which is a hard-wood stopper or cover, D, cemented on with sealing-wax, &c., and through a hole in the centre of which is a glass or other insulating tube, C. Through C passes a brass rod, B, secured to a brass plate (or ball), A, at the top, the lower extremity being flattened out or attached to a flat cross-piece of metal, E. On to the sides of E gold-leaves or pieces of Dutch metal, L L, are gummed, or otherwise attached.

Ex. CV.—A somewhat more elaborate instrument is shown in Fig. 59, in which A is the plate, B the insulated rod, and L L the leaves. In addition a metal shield, G, is fitted to the upper portion of the glass jar. It is insulated from the rod and plate A, but is connected to the earth or some other conductor in connection with it. Two strips of tinfoil, K K, are gummed or pasted on the inner surface of the jar, reaching at least high enough for the leaves, L L, to touch them instead of the glass (in the event of their being so far separated). These strips are also connected to earth. A small glass tray, H, of any convenient shape or dimensions is laid on the wooden base, W, and is supplied with a few small pieces of pumice-stone saturated with sulphuric acid, the function of which is to absorb any atmospheric moisture that may be present.

Ex. CVI.—A form frequently recommended for amateurs or beginners is that illustrated in Fig. 60, where F is a glass flask, which has been thoroughly dried and warmed, and in the neck of which is a sound cork or vulcanite stopper, C, through which is passed a wire, W, the upper part of which is soldered to a small disc, T, the lower part, W′, being bent at right angles to carry the leaves, L L.

Ex. CVII.—For our present purpose I have not a great opinion of either of these pieces of apparatus. They are one and all more costly or more elaborate than is necessary. The object of the glass jar or flask is simply to protect the leaves from air-currents, and prevent their being affected by atmospheric moisture. If we could get a transparent metallic jar, nothing could suit our purpose better, but that, of course, is out of the question. Let us study Fig. 58 and its teachings a little, and we shall then see what is actually required. Suppose an electrified rod, say of glass, to be brought near the plate A. Induction takes place, A becoming negatively, and the leaves L L positively, charged. L L, in consequence of their similar charges, mutually repel, and, instead of hanging vertically, take up positions more or less resembling those illustrated, the amount of divergence

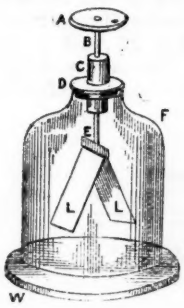


Fig. 58.

Fig. 59.

Fig. 60.

in any particular instrument depending naturally upon the charge. On the electrified rod being withdrawn, the leaves fall together, exhibiting no signs of electrification, the equal charges produced at A and L L combining and producing neutrality again. Let the rod be once more brought towards A, and the electroscope re-charged; then (the position of the rod being unaltered) let the finger touch B, or any part of A not immediately opposite the rod. The leaves will fall together consequent on the positive electricity repelled to them being neutralised by the earth-contact through the finger. As a matter of fact, the state of affairs is somewhat akin to a huge conductor, of which one extremity is at A and the other at some antipodean point.

Remove the finger. No change takes place in the position of the leaves. Subsequently remove the glass rod and the leaves diverge: this time with negative electricity, because the removal of the rod "sets free" the negative charge concentrated at A, some of which charge finding its way to the leaves causes the divergence. A finger placed on A neutralises this negative charge. Suppose the finger not to be so placed, then we have a charge of electricity in the leaves whose quality (positive or negative) we know, because we first used a body provided with a known charge.

To determine the quality of another charge all that is necessary is to place the body carrying the charge near A, and inducing a further charge in the metallic system A L. If the unknown charge is negative, a positive

charge collects at A, and additional negative is driven into the leaves, whence the previously existing divergence is increased. An increased divergence then proves that the unknown charge is similar to the known charge in the leaves. The falling together (or collapse) of the leaves may indicate an opposite (in this case positive) charge on the body being tested, but there is a little chance of our being deceived in this case, as the approach of a neutral body might produce a like effect, for the charge in the leaves would speed to the assistance of the similar charge on A in its efforts to electrify the previously unelectrified or neutral rod. This may be proved by bringing the hand near A.

With a ball or plate at A (instead of a metallic point, or the end of the wire) the charge on the leaves may be retained for a considerable time, but this advantage is lost if there are any points on the metallic system, or if the air confined in the jar has not been thoroughly dried. The necessity for dry warm air is apparent. There is some danger that, in the event of the charge on the rod under examination being exceptionally strong, the leaves will be made to take up positions so far divergent that they enter into contact with the surface of the glass. Now, the presence of a charge at L L indicates the ability to induce a charge on the surface of F. This certainly takes place when mutual attraction between this charge and that on L L sets in. If the surface of F is but a poor conductor, the leaves are liable to stick. This is overcome by gumming or pasting two strips of tinfoil on the inside of the glass, as K K (Fig. 59), and placing them in connection with the earth. The charge upon L L readily produces an opposite charge upon K K, the similar charge pushing its way into the earth. The divergence of L L is accordingly increased, and possibly L L comes into contact with K K, in which case neutralisation immediately ensues, and the leaves fall together uninjured. The metallic shield, G (Fig. 59), is to prevent the electrified rod exerting any direct inductive effect upon L L. The function of the sulphuric acid, with which the pumice-stone in H is saturated, is to absorb any atmospheric vapour that may be present.

The objections to the form depicted in Fig. 60 are apparent. In the first place, no tin-foil strips can be utilised, and, in the second place, it is impossible to remove any atmospheric moisture, dirt, &c., which may have been left behind in sealing up. We will next turn our attention to an instrument the cost of the materials for which shall not exceed threepence, but which, nevertheless, will answer our every purpose.

THE GREAT RED SPOT ON JUPITER.

BY RICHARD A. PROCTOR.

(*Continued from p.* 157.)

LET us look into this matter a little more closely: and first, let us ask if anything akin to the difficulty thus recognised in the case of Jupiter (and also in that of Saturn) exists elsewhere.

Now in the case of the sun we have an orb which is probably in large part gaseous. We certainly have, *visibly*, a gaseous region thousands of miles in depth, even estimating the depth only from the visible surface of luminous cloud which we call the photosphere. And in the sun's case the attraction of gravity on the atmospheric region thus recognised, is ten or twelve times greater than the attraction on the atmosphere of Jupiter. Therefore we have in the sun's case a much greater difficulty than in the case of Jupiter or Saturn.

It is true that the intense heat pervading the whole frame of the sun suggests a way of meeting the difficulty which does not at first sight seem available in dealing with the giant planets. The laws which connect density and pressure at ordinary temperatures and at ordinary pressures may probably fail altogether where the temperatures are so high and the pressures so enormous as they must be throughout the whole frame of the sun. We may say, indeed, as I have elsewhere shown, respecting the outer parts of the sun we see, what Professor Young said of the usually unseen corona, that if the term atmosphere be understood as we understand it when speaking of our own air, the gaseous regions forming the parts of the sun next within the photosphere do not form an atmosphere at all. Here are his remarks in regard to the corona, each one of them being fully applicable to the gaseous envelopes within the visible surface of the sun:—"Granting for the moment that the corona is in part and largely composed of an envelope of exceedingly rare gaseous matter around the sun,—then we may call it an atmosphere, because being gaseous and attached to a cosmical body, it bears to that body a relation analogous to that borne by our atmosphere to the earth itself. So far the term is a proper one. But now further, and on the contrary, the term 'atmosphere' carries with it to most persons certain ideas as to the distribution of temperature, density, &c., in its different parts, which are based on the fact that our terrestrial atmosphere is nearly quiescent and in static equilibrium under the force of gravity, with a temperature not more than two or three hundred degrees above the absolute zero, while the density of the portion accessible to human observation is very considerable. On the sun the conditions are immensely, and almost inconceivably different, so that the term 'atmosphere' becomes a very misleading one. There the equilibrium, so far as there is any, is dynamical, not statical, and the density, temperature, and condition of the gaseous substance is far more nearly that of the residual gas in a Crookes's vacuum tube through which an induction coil is sending electrical discharges; so different from that of ordinary air that Crookes thought he had found a fourth state of matter, bearing some such relation to the gaseous state as the gaseous does to the liquid."

That this is so in regard to the sun is shown at once if we remember that the great openings we call spots disclose solar regions lying certainly not less than 10,000 miles below the sun's visible surface. Now the strength and breadth of the hydrogen lines seen in the spectrum of the sun's coloured flames show that the hydrogen present there is not indefinitely rarer than hydrogen at the pressure of the air we breathe. Putting the pressure at the sun s visible surface at the millionth part of the atmospheric pressure on earth at the sea-level, and noting that gravity at the sun's visible surface is 27 times gravity at the earth's, we find that at a depth of two or three miles below the sun's apparent surface atmospheric pressure would be the same as at our sea-level were the same gases present, and temperature the same there as here. For in about the eighth of a mile the pressure would double, so that in 2½ miles there would be twenty doublings of pressure, raising the density from the millionth part of our air's to somewhat more than equality with the density of our air (2 doubled, that double doubled, and so on, to 20 doublings, giving 1,048,576). In the next 2½ miles the pressure would be increased more than a millionfold,— always assuming

the conditions to hold which we recognise in our own atmosphere. This would happen in five miles out of 10,000 miles of depth, known to be occupied by gaseous matter.

Even taking into account the tremendous heat prevailing in the sun, and the existence of much lighter gases in his surroundings than exist in our own air, we cannot escape conclusions scarcely less preposterous and assuredly quite as inadmissible as we have thus reached. If the pressure and density did not double in less than a mile, or than ten miles, or even a hundred,—which is altogether impossible—we should still have, within a range of 10,000 miles, 10,000, or a 1,000, or a 100 doublings (in these cases respectively); and consequently even with the least of these numbers there would be a density at the base of the 10,000 miles exceeding a billion billion times * the density of our own air.

Undoubtedly it is not in the high temperature of gases near the sun, or not in this only, that the solution of the enigma lies. We have also to take into account the freedom of movement which exists throughout the gaseous envelopes of the sun, and the constant movements which are no doubt taking place within these envelopes.

In some such way, I think, we must encounter the difficulty, kindred in character if not so great in degree, which exists in Jupiter's case. We must admit the existence of intense heat throughout the gaseous surroundings of Jupiter, though we need not imagine that they are as hot as the gaseous envelopes of the sun, or that their temperature ever approaches solar temperatures. We must admit great freedom of motion within these gaseous and vaporous regions around Jupiter. So may we at once escape the difficulty which Jupiter assuredly presents, and be led to the conclusion which we had already reached from another side,—viz., that Jupiter's outer portions to a depth of many hundreds of miles within his visible surface do not belong to his real globe, but are mainly formed of gaseous, vaporous, and cloud-like matter.

From yet other directions the same result has been reached, as I pointed out in my "Other Worlds than Ours," many years before the great spot had appeared. No one now supposes that Jupiter is made of other materials than those which form the earth on which we live, nor does any one now suppose that Jupiter is a hollow planet as Sir David Brewster insisted. Yet if we do not adopt one view or the other we cannot possibly explain the small mean density of Jupiter otherwise than by assuming that the globe we measure for Jupiter is very much larger than the planet itself. Jupiter is 1,250 times as large as the earth, but only 310 times as massive. This, alone, proves that the real globe of Jupiter lies far within the cloud-strewn surface we measure. With the enormous attraction residing in 310 times the earth's mass, a globe of the same materials as our earth would be considerably denser instead of less dense than the earth. Assigning to Jupiter a density only equal to the earth's its diameter would be little more than 50,000 miles. Jupiter's diameter is fully 80,000 miles. The distance of the cloud-strewn surface we see, from the real surface of the planet, cannot then, it would seem, be less than 15,000 miles (the difference between 40,000 miles and 25,000 miles, the halves of the just-named diameters).

The telescopic aspect of Jupiter corresponds much better with this startling result than with the idea that he has an atmosphere in the least resembling our earth's.

* In the first twenty doublings equality with our atmospheric pressure would be attained, in the next twenty the pressure would be a million times greater, in the next a billion times, in the fourth twenty doublings the pressure would be a trillion times, and in the last twenty it would be raised to a quadrillion times the pressure at our sea-level. (I use the English system of numeration according to which a million raised to the second power is a billion, to the third power a trillion, to the fourth a quadrillion, and so forth.)

BAUDRÉ'S SILEX PIANO.

AMONG the flint stones that are met with in the chalk formation there are some that when struck with another flint emit sounds of great purity. The tones that are thus obtained with different musical flints are out of all proportion to the bulk and weight of the stone. This is a very curious phenomenon, the explanation of which is not furnished by the fundamental laws of acoustics, and which surely merits being studied by physicists.

As long ago as 1873, I spoke of musical stones as a curiosity worthy of attracting attention. I then promised to return to this interesting subject, but the years passed by, and the singing stones were forgotton. Upon recently visiting the new electric lighting of the Grévin Museum, however, they were casually brought to mind again. After examining this interesting installation, I was walking through the great hall of the museum, looking at the wax figures mounted therein, when I heard some delightful music that attracted my attention. Approaching the spot where these harmonious and pure sounds were being produced, I saw a musician, who, holding two flints, was playing upon a stone piano with wonderful agility, by striking other flints of all shapes suspended by two wires at a few fractions of an inch above a sounding-board. I at once made the acquaintance of the player, who was Mr. H. Baudré, a distinguished musician and a zealous collector of musical stones.

"How did you procure these flints that render so delightful sounds, and from which you get so remarkable music?" said I.

"Ah, sir, it required much time and many trips to collect the twenty-six stones which you see before you, and which form the two chromatic octaves. It took me more than thirty years (from 1852 to 1883), to search for them in the chalk-beds of Haute-Marne, Périgord, Eure, and the Paris basin."

"Are such flints found in all chalk formations?" "I believe not; the innumerable quantities of English flint have yielded me nothing acceptable." "Are there any works that treat of this interesting subject of singing stones?" "I do not know; but I have letters from numerous scientists, who have been pleased to congratulate me, or to give me their opinion."

"Would you communicate a few of them to me? I should like to publish them in *La Nature*." "Very willingly, sir; I will send you my file to-morrow."

The following are a few of the notes that appear to me to give some new information in regard to singing stones.

M. Cartailhac, director of the Toulouse Museum, reports that three musical flints were once noticed by a missionary in the village of Chaffa, in the centre of the plain of Thumazana, Abyssinia. These stones were hung by threads from a horizontal wooden rod, and were used for calling the faithful to prayers or to battle. They were struck with another flint, and their sounds, which were very intense, were heard from some distance.

In an interesting letter to M. Baudré from Mr. J.

Ellis, member of the Royal Society of London, this learned scientist treats of the sonorousness of singing stones. "We know not up to the present," says he, "whether the sonorousness is affected by the form, bulk, chemical mass, or molecular constitution. It is very probable that these stones have internal structures that differ from each other—the sound of the stones being different when they are struck in two neighbouring places. I should not be surprised if there were a sort of obliqueness in the structure, which would explain the impossibility of preserving the sound when a singing stone is cut or broken.

"There is here an interruption of the sonorous waves that are passing through the body. The great difference in the sounds that two bodies of nearly equal bulk are capable of producing is probably due to a difference in the arrangement of the molecules, which govern the mode of vibrating. I am sorry that I am unable to say more on this subject."

M. Baudré calls his singing stones "prehistoric music." It is not impossible, in fact, that analogous keys were used by our ancestors of the Stone Age. This was Abbot Moigno's opinion.

"Who knows," says the old editor of *Cosmos*, "whether, in eagerly excavating in search of relics of the Stone Age, we shall not find a series of attuned flints? Why may not the flint, which was the first arm, the first tool, of pre-historic man, have also been his first musical instrument?"

M. Baudré thinks that the reason no musical instruments have been found in prehistoric strata is that searchers have not occupied themselves with native flints, but only carved ones.

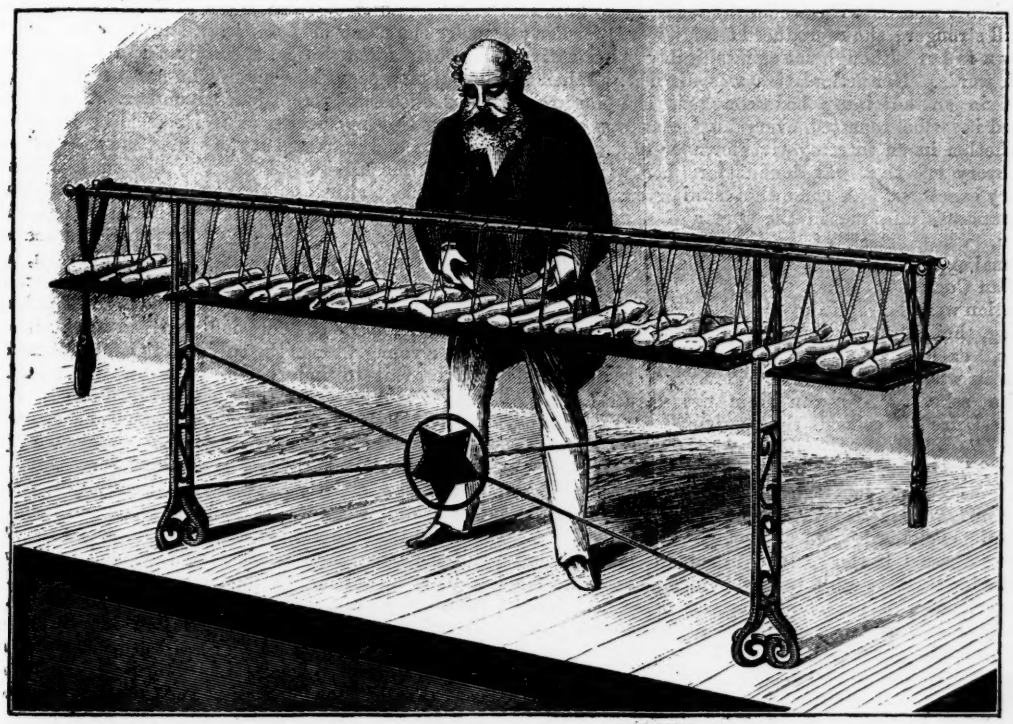
The following are some of the peculiarities of these attuned stones: The stone that emits the greatest tone weighs 4½ lb., while the one that gives the half tone of this weighs 9. This large flint is immediately followed by one of one ounce, that finds its similar in weight only at the end of the series, although the difference in sound is considerable. A three ounce stone gives exactly the same note as another that weighs but 6,000 grains. It will be seen that we have surprising anomalies here to puzzle physicists.—*G. Tissandier, in La Nature.*

THOUGHT AND LANGUAGE.

By ADA S. BALLIN.

XVI.

IN my last article I alluded to the fact that those characteristics acquired latest in the race and in the individual are the first to be lost in cases of degenera-



tion. Thus the child walks on all-fours before he acquires the power of walking erect; and, occasionally, idiots are met with who can never be taught to walk otherwise than as quadrupeds do. Sometimes the patient seems to belong to a lower stage of evolution to that into which he is born, and animal instincts and propensities are linked with incapacity for the use of language. Although I have, unfortunately, not been able to obtain further particulars, I take it that the following cases belong to this class.

"F. F. G.," in the *Lancet*, Feb. 21, 1885, mentions a child, aged nine, who is one of four or five, none of whom can speak intelligibly—saying only a few simple words, and those only half articulated. This child has had no illness, but is very perverse, and developes frequently a strong tendency to mischief and destruction: tearing clothing, breaking windows and furniture, &c. Such fits last about a day; in the intervals she is very

intractible and disobedient. He regards them as evidences of epileptiform seizures, with tendency to insanity. The parents are related (cousins, I believe); no other family history. It seems to me that this is an instance of pure mental atavism—reversion to an ancestral type somewhat resembling that of the monkey, brought about by congenital nervous defect combined with disease.

In cases of brain-disease where the intelligent use of language is lost or impaired, when words are used in the presence of the patient, they are frequently imitated and repeated by him in a meaningless way. Prof. Behier* had a patient who was born in Italy and had lived in Spain and France; she thus mastered three languages, but lost the use of Italian and Spanish, and only retained a most limited power over French, in which she could *only repeat like an echo*, and without attaching any meaning to them, the words pronounced in her hearing. Bateman, the celebrated writer on aphasia, saw in the Salpêtrière a woman with whom the tendency to imitate was still stronger; she reproduced foreign words formerly unknown to her, although not as intelligibly as her native French words. Her articulation was distinct. When a patient in an adjoining bed coughed, she instantly imitated it. She repeated everything that was said to her, whether in an interrogative form or not, and imitated every act that was done before her with extraordinary exactness. A somewhat similar case occurred in Trousseau's clinique in the person of a man named Marcou, whose stock of words was limited to the emotional expressions, "My faith!" (Ma foi!) and "Cré nom d'un Cœur!" When asked what his name and his occupation were, he replied, "My faith!" On Trousseau's insisting, he only shook his head with an impatient gesture, exclaiming "Cré nom d'un Cœur." As Trousseau wished to discover how many words he could use, he asked, "Are you from the Haute-Loire?" "Haute-Loire!" echoed the patient. "Your profession?" "Haute-Loire." "But your name is Marcou?" "Yes." "You are sure it is Marcou?" "Yes." "What department do you come from?" "Marcou." "No; that's your name." With an impatient gesture he exclaimed again, "Cré nom d'un Cœur."

As we descend in the scale of human intellect we find that the tendency to imitate has increased power. We all experience this tendency ourselves when not intellectually active, as when a cough, a sneeze, or a yawn goes all round a church, lecture-hall, or concert-room;† but in nervous disease and among peoples of a low class of intellect, such as savages or uncultivated peasants, it is very much more marked. Among the lower classes of various nations this mental phase is seen in exaggerated form. The "Jumpers" of America will obey any order, however absurd, and repeat words in languages of which they are entirely ignorant. Among the Russians it assumes the proportions of a nervous disease, and is called "Miryachit." The patient has no power to resist imitating actions which he sees or hears performed by others, such as clapping the hands, shouting, or even stumbling and falling. In his "Journey to the Amur," Dr. Meak says that it is not unusual for the Maniagri to suffer from a malady of this kind, which is met with chiefly among the wild people of Siberia, but also among the Russians settled there. It is also common among the Yakutes and Argurian Cossacks; those suffering from it under the influence of fear or consternation unconsciously imitate whatever passes before them. I can only look upon this so-called disease as a symptom or evidence of nervous degeneration, and the statement that the imitation is more frequent when the patient is terrified, and, as we may call it, "nervous," justifies this opinion. In the same way in slight cases of chorea we find the involuntary movements which characterise the disease increased by nervousness, as when the patient is being questioned about the disease or asked to perform certain voluntary movements of which she fears she is incapable.

Mr. Jagor observes in his "Travels in the Philipines,"* that this disease of nervous mimicry is well-known in these islands under the name of *Mali-Mali*, and in Java under the name of *Lakit-latar*, and he tells how he and his companions availed themselves "of the diseased condition of a poor old woman who met us in the highway, to practise some rough jokes upon her. The old woman imitated every motion as if impelled by an irresistible impulse, and expressed at the same time the most extreme indignation against those who abused her infirmity." Dr. Neale, writing on the same subject, says that whatever attitude is assumed these individuals must adopt the same. If they are carrying anything and see another person drop an article, down goes their burden, however precious and previously carefully carried, even if it is a valuable piece of crockery or a well-loved infant. He continues, "On one occasion I wished to extract a tooth from a young woman the subject of 'lata,' but no coaxing could avail to induce her to open her mouth. At last I took a chair, and she did likewise, I moved it gradually towards her in front, and she as gradually approached me. I then yawned very widely; she followed suit. I then shut my eyes very tight, which action she strictly imitated. Quickly jumping up I clapped on the forceps, and had the tooth out before she had time to resist further." Some years after this girl came to England as a servant, and one day, while waiting at table, her mistress wished to exhibit her peculiarity to some friends, and seizing a large French plum pretended to swallow it. "The woman instantly made a dash at the dish, thrust a plum in her mouth, and, after much choking and semi-asphyxia, succeeded in swallowing it; but the situation was such that her mistress did not repeat the experiment."

It is well known that healthy children will often acquire chorea solely from seeing others afflicted with the disease; but one of the most forcible illustrations I can imagine of the power of imitation is that afforded by a report of the Massachusetts Asylum for the Blind, in which Dr. Howe observes that a blind child during the vacation got St. Vitus' dance, and that when the school reassembled, the disease spread so rapidly among the children that, in order to arrest it, they had to be separated and sent home again. The choreic movements must have been imitated wholly through touch.

Physiological action increased beyond the normal intensity becomes pathological action, and thus we see that imitation, which, as we descend in the scale of human intelligence, becomes more and more a physiological unit of the constitution, may pass with great facility into a well-marked disease.

* "Gaz. de Hôpitaux," May 16, 1867.

† "Laughter is catching," as we all know. As a rule, if an actor on the stage laughs, the whole audience involuntarily copies him, whether the joke is generally understood or not. A short time back I went to German Reed's, and saw "Hobbies," a piece in which one of the actors becomes possessed of a "laughing gas" machine, which makes him go into ecstasies of laughter; the audience was convulsed, and although I was anxious to watch this phenomenon in the interests of psychology, it was perfectly impossible for me to withstand the infection, as it may well be called.

* p. 159.

The lower we descend in the scale of human intelligence the nearer do we approach the highest types of animal mind, and accordingly we find that the faculty of imitation is largely developed among the more intelligent animals, and is at its highest in monkeys. In the human infant it begins very early. Preyer says that the first imitative movement begins as early as the fifteenth week, when the child copies the action of protruding the lips if it is performed before it. Towards the end of the first year imitative movements become more numerous and more quickly learnt. At twelve months Preyer saw his own child repeating in its dreams imitative movements which had strongly impressed it when awake; for example, blowing with the mouth. Later still, complicated imitative movements are repeated for amusement, as seems to be the case with monkeys. "With growing intelligence (as Romanes says) this faculty subsequently declines, and in after life may be said to stand in an inverse relation to originality or the higher powers of the mind. Therefore among idiots below a certain grade (though, of course, not too low), it is usually very strong, and retains its supremacy through life, while even among idiots of a higher kind, or the 'feeble-minded,'* a tendency to undue imitation is a very constant peculiarity. The same thing is conspicuously observable in the case of many savages, so that in view of all these facts we must conclude that the faculty of imitation is one very characteristic of a certain area of mental evolution."†

OUR HOUSEHOLD INSECTS.

By E. A. Butler.

COLEOPTERA (*continued*).

WE have only one other group from which to select examples of British household Coleoptera. These are the most highly developed of all, the Geodephaga (or predaceous ground beetles, as the name signifies), which in classifications are placed at the head of the whole order. Two species of this important group are found in cellars and dark outhouses. They closely resemble one another in shape, but, nevertheless, may easily be distinguished both by size and colour. The larger, *Sphodrus leucophthalmus* (Fig. 1), which is also a bakehouse insect, is black, and the smaller, *Pristonychus subcyaneus* is, as its trivial name implies, of a steel blue colour. Both are exceedingly active, as well befits creatures which carry on a perpetual warfare against their smaller and weaker brethren, and subsist by rapine; indeed, the whole section Geodephaga are renowned for the extraordinary agility of their movements, in which characteristic they surpass all other Coleoptera, and in conformity with these habits, their legs are long and slender. The name *Sphodrus* is Greek for the "active, violent, or vehement one," and so far as mere meaning is concerned, would be equally applicable to almost all the Geodephaga.

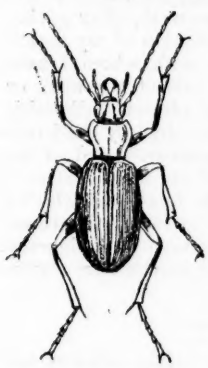


Fig. 1.—Sphodrus leucophthalmus (natural size).

Sphodrus is a fine insect, fully an inch long; but *Pristonychus* attains little more than half this length. They are so similar in shape that a figure of one will be quite sufficient to enable both to be recognised. The most elegant part about them is the thorax, which is of the form called by entomologists "heart-shaped"—that is, the outline of the margins consists of a double curve on each side, the front half being convex and the hinder concave. This has the effect of forming something like a waist, and of imparting an air of neatness and refinement, so to speak; and, therefore, even *Sphodrus*, though so large, can certainly not be considered either coarse or clumsy.

A formidable pair of jaws, with which the insects can give an unpleasant nip, if incautiously seized, project in front of the head like a pair of shears. The pair of small jointed appendages, like two minute antennæ, by the side of these, are the maxillary palpi, or feelers attached to the maxillæ, or secondary jaws, which underlie the mandibles or true biting-jaws. The insects lurk under stones and in dark corners, and if suddenly disturbed in their hiding-places, make the most frantic efforts to recover their shelter. They can always find plenty of food, in the shape of the other cellar Coleoptera, to which we have already referred, and many other kinds of insects that frequent such situations.

Besides the more legitimate coleopterous inhabitants of our houses, which we have described in the preceding articles, there are, of course, plenty of stray visitors that may at any time turn up. On a fine summer day, windows into which the sun is shining brightly often have a considerable insect population, amongst which many beetles may be found, especially such as ladybirds and tiny rove-beetles. Sometimes great rarities may be met with in this way. Thus the Rev. W. W. Fowler records having taken, off a lodging-house window at Hunstanton, two minute beetles which had scarcely ever been met with in Britain before, beetles which were not household species at all, but simply casual visitors which had flown in and been unable to find their way out again. The corridors of the Crystal Palace, similarly, often yield hosts of tiny beetles, and there is one species that is specially noted as occurring there.

Our food, too, sometimes introduces us to more insects than we care for. House-flies, drowned in milk or soup, or fossilised in bread, and caterpillars boiled with peas or cabbage, will, of course, at once occur to one's mind; and, indeed, considering the inquisitive nature of some of our household pests, it is a marvel that we do not get more of them served up to table than we do. Foreign beetles sometimes occur in brown sugar, easily hidden amongst the crystals as long as the sugar is in the solid form, but brought into undesirable prominence when it has been dissolved in our coffee; the only specimen my cabinet boasts of a certain species that is now reckoned as British, though, no doubt, originally imported, was thus fished out of a cup of tea. And it is not merely *small* insects that appear in this way. I once met with a fine large South American weevil in a gooseberry tart; it was a handsome species, of a purplish-brown colour, with some clear yellowish-white circular spots, and was about half-an-inch long. It was in excellent condition, and not in the slightest degree damaged by the cooking it had undergone, but able to be set up as a perfectly respectable cabinet specimen. I have known, too, of an instance in which the shell of a pond-snail came to table in a loaf, apparently having reached so strange a position through the medium of the water used in mixing the dough.

Beetles, too, may be found on our doorsteps, or climbing the walls of our houses. I once met with two specimens, the only ones I possess, of a rare beetle, one, in fact,

* See the case of the deaf and dumb idiot negro boy cited in Article XI. of this series, KNOWLEDGE, No. 190.

† "Mental Evolution in Animals, 1883," p. 225.

which has only of late years been recognised as British, climbing up a pillar just outside the front door of a house. Many may also be found in thatch: if an old thatched roof, which has become black with age, be beaten with a stout stick, and the dust and rubbish that come out be caught in a bag, and afterwards carefully examined by shaking it over a piece of white paper, the beetles it contains will easily be seen; they are mostly exceedingly small, but some are very interesting forms. In birds' nests built in the gutters or under the eaves, and in pigeon-cotes and fowl-houses, many species also habitually live. If a quantity of the filthy refuse that accumulates on the floor of a fowl-house be shaken over paper, hundreds of beetles will come tumbling out, some of them even sufficiently valuable to make amends for the unpleasantness of the method of obtaining them.

In country-houses, if an old log be put on the fire, any insects it may contain, speedily finding their quarters becoming too hot for them, make their escape, and as often as not find their way to the window, where, of course, they are readily observed and may be easily captured.

Here we may conclude the consideration of the first order of insects, the Coleoptera; in the next paper we shall proceed to notice the second order, the Hymenoptera.

(To be continued.)

Gossip.

By Richard A. Proctor.

After the last number in September Knowledge will be published as a monthly magazine, price sixpence.

Like other monthly magazines, Knowledge in future will not be open to correspondence properly so called. Communications addressed to us by readers, in regard to subjects of interest with which they are conversant, will of course be welcome as they have always been. But argumentative letters would manifestly be out of place in a monthly magazine; indeed I imagine that argumentative persons would not be content with so slow a method of conducting their discussions as a monthly magazine would afford.

I am obliged to admit that the introduction of the correspondence element, and especially of the replies to correspondents, was a mistake from the beginning. We have had communications of great value from several correspondents, and among the questions asked have been many which it has afforded myself and others pleasure to deal with. But the trouble has arisen which always, as it seems, must arise in such cases,—the difficulty of excluding correspondence of the argumentative sort, suggested not so much by any desire to get at the truth as by the wish to air theories which have had their origin in ignorance or misapprehension. The trouble is not so much that correspondence of this sort occupies space which can be ill spared, as that it tends to drive away, gradually but surely, such correspondents as really have interesting matter to communicate. I have to thank several able writers for the kindness with which they have resisted the natural tendency to withdraw from correspondence leading to controversy, and sometimes to controversy of the idlest possible kind. But the influence of the sense of discomfort thus occasioned must tell in the long run. I know this from my own experience, remembering how I was compelled to withdraw from the correspondence columns of the *English Mechanic* because of the association into which I found myself brought with paradoxists, flat-earth men, weather prophets, circle-squarers, and others of like sort. Controversy equalises in the eyes of outsiders the ignorant and the well-informed, and the ignorant know this. Science suffers by such controversy, because well-established facts are made to appear as if they were subjects for discussion, where in reality they have simply been misunderstood. In the columns of Knowledge, hereafter, scientific, literary, and artistic matters will be dealt with by those who know, not discussed between those who know and those who do not know.

With regard to questions, I simply cannot longer afford time to answer them in Knowledge. I have had great pleasure in replying to legitimate questions, and so I am sure has my friend the acting Editor. But questions have been poured in which are not legitimate. We have been asked to solve problems given in examinations,—as if we were in the private tutor business. We have been asked to explain matters which should be studied in text-books. Men so ill-informed or so limited in brain-power that they cannot understand simple explanations, have started out as original discoverers, propounders of new theories of the universe, and so forth, abusing us for not at once accepting their perplexities as new and brilliant lights. Then among the letters sent to us (in company, be it always admitted, with others that are most pleasing) are some giving vent to the hee-haws of vulgar buffoonery. "Jolly Dogs" (wanting knowledge, but what they want with Knowledge we do not find) send letters smelling of stale tobacco and suggestive of beery influences, beginning (for example) "You'll be the death of me, I know you will," and going on, perhaps, to ask who on earth believes the untruths (monosyllabic) which geologists, chemists, or astronomers are always telling,—making special reference, it may be, to some of the most elementary truths in the sciences jeered at.

All this would not in the least matter if the work of answering correspondents were in itself light. But in reality the work is not light; it has taken time which could very ill be spared: it has in fact involved a costly expenditure of both time and labour, for which I have had no return. I withdraw from the work, after giving it four years' fair trial, with the expression of my thanks to the many kindly correspondents with whom I have been brought into contact in this part of my work on Knowledge, of my regret that our pleasant fellowship should have been in some degree impaired by the irruption of persons ill-informed or ill-mannered, and of my sorrow that apart from this I could not have continued the work. I have much work to do, or at least which I wish and hope to do; my health and strength are not what they have been; and I must devote what time and capacity for work may remain to me, to the more serious business I have in hand.

I remark however, that as the Gossip columns will not only remain open but be extended, questions relating to matters of general interest may still be dealt with in a form useful to the general reader. I *promise* nothing in that direction, partly because I do not want to invite questions, and partly because I cannot tell what time I may find available for such matters.

In the nature of things Knowledge as a monthly magazine must be an experiment. I shall spare no pains to make it a success. I hope friendly readers will co-operate, as they can very readily and effectively do, by announcing to others the new departure. The form in which Knowledge has hitherto appeared in regard to size of page will be retained; but as there will now no longer be that necessity for rapid work which the weekly number had involved, Knowledge will present in many respects an improved appearance. Original articles will occupy a larger relative proportion of our space, which will of course be also absolutely larger. The complaint which some have made of the scrappy form in which our leading subjects appear will no longer be justified. As to the quantity of matter, original and select, in each number, comparison can be confidently invited with any monthly or weekly serial, not backed by novels or short stories, or padded with mere business communications or the long-winded drivel of paradoxists and their kind.

I hope to receive for "Knowledge, a Monthly Illustrated Magazine of Science, Literature, and Art," such support as will amply justify its continuance in the course entered upon. I will not say less; but there is so much other work inviting me that (remembering duties nearer to me than those I owe to science) I cannot at present say more.

We are going to change our course somewhat freely in another respect. At the outset of our career, we expressed a determination not to deal with the influence of science on religion, though satisfied that that influence had been purifying and wholesome. This was regarded by many as a promise. In reality it was a precaution. We wished to escape the flood of controversy, which we knew would pour in upon us if that subject were opened. That we did not escape, a glance at the "Replies to Correspondents" will serve to show; but we kept our correspondence columns tolerably clear of the *odium theologicum*,—which was our principal object. But now our position is different. We can present the views of science in regard to religion without introducing controversy. We can answer the questions which are repeatedly being asked as to what science can do to replace the religious feelings which many seem to think, mistakenly enough, that they must abandon if they would tread the scientific pathway.

You give us, say many, the Everlasting No, in which is neither hope nor solace, neither help in the work of life nor promise of better things hereafter. Is science without religion and without hope? Must the votary of science be without faith, without worship, without a law of duty?

We shall endeavour to show that the teachings of science involve no such gloomy and hopeless picture. Science answers with an Everlasting Yea, the questions of those who desire to possess faith, to feel reverence, and to recognise the sense of duty and right.

The great philosopher of our age is not anxious to be followed by a train of disciples; he would preach no new religion. But he knows, what others have felt, that the purifying of old religions from the dross of ages is no destructive process. Through the infinite azure depths of the cleared sky the real glory of the universe is beginning to be seen. Purified—even it may be to perfect transparency,—religion will remain religion still. It will have its temples, but temples not made with hands; its worship, but a worship cleansed from all that is unworthy; its code of morals, but a code based on reason and on justice. One characteristic alone, which has been associated with religion, the religion taught by pure science will not possess. Its very essence will be freedom from all intolerance. Because it recognises in all true forms of religion a yearning after good, a desire to feel the presence and power of something outside of us that makes for right,—science can be intolerant only of intolerance. The religion of science is indeed in harmony with all true-aiming religions, discordant only with what is self-discordant,—the jarring voice of cruelty and hatred.

We propose to begin, in our first monthly number, a series of papers in which the positive aspect of the teaching of science in regard to religion will be considered. We had intended to bring out this series of papers as by Thomas Foster, our *alter ego;* but as it is now an open secret that Thomas Foster and R. A. Proctor are one, the better plan will be to put over the articles the proper name of their writer. The use of a second name has served its purpose, in securing for articles outside science, attention which the old rule "*Ne sutor ultra crepidam*" would have turned from them. Mr. Foster's articles on "Happiness" for example were published widely in America (and handsomely paid for, let me remark in passing) by the editor of the *Popular Science Monthly*. But there were inconveniences in the plan. For instance, Mr. Foster was invited by one of the most eminent Spencerians in America, to meet other admirers of Mr. Herbert Spencer, including Mr. Proctor, at a public gathering,—an invitation which he could not readily decline yet could not possibly accept without duplicity.

Hereafter, therefore, Mr. Thomas Foster disappears from these pages. (It should have been Forster,—from an old family cognomen Forster Thomas, by which the eldest son in my family has been called for many generations, but I had forgotten how the name was spelled,—not so remarkable a forgetfulness as it might be supposed, seeing that the last owner of the name died twenty years before I was born.)

"A Father" writes indignantly thus respecting lawn-tennis as played by his daughters:—"I have noticed that after a game of lawn-tennis my girls appear to be almost exhausted, they perspire profusely, and are susceptible to draught. Their sleep is disturbed because of their excessive weariness, and they have several times been lamed and used-up. I have finally forbidden them to play lawn-tennis. I am not going to have my womenkind laid up with sprained ankles and twisted wrists, strained tendons and colds in the head."

Quite right, papa. The best tennis-players of the masculine persuasion will not be angry with you for keeping your girls away. It is obvious from your account, read between the lines, that your "womenkind" have tried to play in corsets and high-heeled boots, getting far more exhausted through their ill-suited habiliments than through the mere exercise. (Though girls *will* overdo such things, just as they overdrive willing horses, and dance too often and too long.) In rational dress lawn-tennis would not be half the work they find it, and would assuredly give ten times as much

pleasure to themselves, to their playfellows, and to lookers-on.

But now hear what the other side says about it. A "London Tradesman" who is himself a player of no mean skill, after saying that he has sold more tennis-balls already in 1885 than he did during the whole of 1884, and that lawn-tennis is the game of the future, adds that women had better leave the game to the men. "Women don't play well," he says. "They haven't got the requisite amount of muscle, and they won't spare enough time to acquire it. Women dress themselves in a way that makes a cripple of the best tennis-player in Christendom. They allow no freedom for their bodies. Until they do without corsets and skirts they must not expect to play tennis well. The only good tennis-players are men, and it is seldom you find an expert at the game who will consent to play with a woman. It spoils his style; it makes him lazy, and when a tennis-player loses style and becomes lazy; he had better throw away his racket."

The bitterest part of this is that about experts not playing tennis with women. Most women fancy they play on the average quite as well as men. But it is only because the good players keep away from them. And now probably even the bad players will cease to play with the girls, because it is becoming a recognised thing that only duffers do that, and no young fellow likes to be thought a duffer. Old fellows even do not care to be thought duffers. Very tender and sweet young curates, with an eye to slippers and braces, may still be found trying to play lawn-tennis with the young ladies of their flocks; sheep gambolling with lambs, so to speak. But will the girls be content with easy victories over these tender ones?

There is hope only, it would seem, in the divided skirt, rationally adopted so as to attract no notice,—nay, my occasional observation of lawn-tennis as played by ladies has suggested that under certain conditions which occasionally arise, the rational dress is calculated to be more seemly than the heavy skirts which render corsets necessary. But I am not an expert in the matter of ladies' dresses, nor have I seen the "patterns of rational dress" which are weekly advertised on page ii. of the Knowledge advertising sheet. I only know how the dress looks when complete, and how much it contributes to grace and freedom of motion.

In the following letter which appeared in the *Times* for August 26, I have called attention to a point of great importance to all who have occasion to communicate with friends across the Atlantic—from either side:—

Risks in Transatlantic Telegraphy.—To the Editor of the *Times*.—Sir,—Out of fifteen transatlantic telegrams which I have had occasion to send, or to have sent to me, during the last six years, three have miscarried. This is too large a proportion of failures, and the public ought to know how much or how little they can trust transatlantic telegraphy as at present conducted. Here are, in brief, the particulars of the three failures and of the compensation—save the mark!—made by the persons responsible. In the summer of 1879 I depatched from the office near Burlington House a telegram to my lecture agent in New York on important business, telling him to wire reply. No reply coming, I wired again, and was told no message had been received. For compensation I received expressions of regret and the price of the original message. Last June I wired from St. Joseph, Missouri, to my lecture agent here, in reply to a telegram of his on important business. Late in July I received a letter from him, saying that the telegram referred to in my letters had not been received. I found it had got as far as London, but had not been forwarded. For compensation I received very courteous expressions of regret from the head post-office, with suggestion that I should write to St. Joseph for return of the price paid for the telegram. On August 6, at 11.30 a.m., I wired from Liverpool (office under North-Western Hotel) a message to St. Joseph, Missouri, in one word; but that word announced my safe arrival, and gave instructions on a business matter of importance to my wife. I have to-day received a letter from her, dated August 8 (not a mistake, for she mentions that guns were firing, &c., because it was the day of General Grant's funeral), from which I learn that my telegram, sent off at 5.30 a.m., St. Joseph time, on August 6, had not been received at mid-day on August 8. I do not yet know what compensation, if any, will be offered. Possibly the telegram may have been since delivered—on August 16, for example, in which case no compensation need be expected.—Faithfully yours, Richard A. Proctor, Eastbourne, Aug. 22.

I would advise all transatlantic travellers, who do not wish to cause their friends anxiety, to refrain from telegraphing their arrival. When their friends expect a telegram, as they would be sure to do if they understood it was the traveller's intention to telegraph, its non-arrival would naturally be understood to show that some serious mishap had taken place. Business communications *known to be such* (from their form, for example) may possibly be exposed to less risk of failure. It is to be hoped so. The two business communications referred to above were not so worded that their business character as such could be recognised. I shall be glad to receive and publish evidence bearing on the trustworthiness of transatlantic telegraphy.

Professor Stokes, in his lectures on light, inclines to the belief that Sir F. Herschel's suggestion may be correct, according to which the development of a comet's tail is due to electric repulsion; and Professor Stokes suggests on his own part that the repulsion may be due to an electric charge on the sun, resulting from the process of condensation in the mist particles of the comet's head. I wonder whether any direct evidence showing that either suggestion is sound will ever be obtained. Professor Tait, of Edinburgh, commenting on Professor Stokes's ideas, expresses his still lively love for the "swarm of cosmical brickbats" theory, which he mistakenly imagines to have been suggested by astronomers in explanation of comets' *tails*. In reality, no astronomer ever supposed the cosmical brickbats (that is only Professor Tait's funny way of saying meteors) had anything to do with comets' tails, near which they have never been seen.

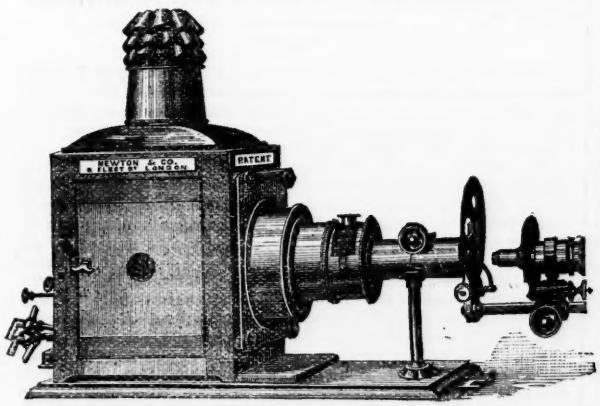
Mr. J. Joly, of the Engineering School, Trinity College, Dublin, suggests as a novelty (I quote from my "Popular Science Column" in the *Newcastle Weekly Chronicle*) the method for avoiding icebergs, which I suggested in 1879, just after the *Arizona* had run into an iceberg at the rate of fifteen knots an hour. "I would like to ask," he says, "whether a thermal radiation method might not serve to show the presence of a large mass of ice in the neighbourhood of a ship." The use of such an instrument as the bolometer of Professor Langley, "or even of the thermopile, in conjunction with a large reflector and an alarm circuit closed by galvanometer deflection, might be worth trial by any one possessing the opportunity." In every detail, the opinion of Mr. J. Joly supports the view that I expressed in the columns of the *Newcastle Chronicle* six years ago. Collisions with icebergs occur seldom, but when they do occur they are apt to be so terrible in their effects, that something might be done, in the case at any rate of our ocean steamships, to warn the sailor of the proximity of the great ice masses which float silently athwart the oceanic roadways.

Our Inventors' Column.

We give here, week by week, a terse description of such of the many inventions as we think may be of use to our readers. Where it is possible, the number of the patent is quoted, to enable those who desire fuller information to procure the specification from the Patent Office in Cursitor-street, Chancery-lane. We shall, generally speaking, confine ourselves to the more recent inventions; but it often happens that an article comes under our notice which, although not quite novel, is worthy of mention for its utility and ingenuity. In such a case we should not hesitate to refer our readers to it. And while we thus increase the interest of our pages, we at the same time assist the inventors by giving greater publicity to their inventions (KNOWLEDGE *being a popular magazine*) *than is accorded by the most excellent trade journals.*

LANTERN MICROSCOPE.

[Patent No. 14,951. 1884.]—This instrument is constructed from the designs, worked out by protracted experiment, of Mr. Lewis Wright, and with it screen demonstrations can be given by the oxy-hydrogen lime-light, of a character hitherto quite unattainable. Ample light is obtained for the magnification of ordinarily transparent subjects to 1,250 diameters, which will display in a clear and beautiful manner all the parts of insects, the minute details of anatomical sections, vegetable tissue, &c., quite as sharply, and almost as brilliantly, as a magic lantern slide; the proboscis of a blow-fly is easily displayed with the various powers from 8 to 14 ft. long. Where transparency of ground is combined with opacity of detail, as in the cornea of a fly's eye, a magnification of 2,500 diameters is attainable. Geological sections are admirably shown either by ordinary or polarised light. The microscope can be fitted to any good optical lantern. In the special lantern constructed for use with the instrument, a triple 5 in. primary condenser is used, which takes up the large angle of 95° of light from the radiant. Almost equal results can, however, be obtained with the 4 in. condensers usually supplied in optical lanterns, by the addition of a third lens, the triple 5 in. condenser being reckoned to give 15 per cent. more illumination. The slides need no special preparation.



The most delicate slides are absolutely and perfectly protected from heat by an inch of alum solution and a layer of Canada balsam, which is not the case with any other lantern microscope. After passing the parallelising lens, the rays are coned down by sub-stage condensers to illuminate an object of the required size. These sub-stage condensers are either single, double, or triple combinations of lenses, according to the power of the objective which is to be used with them, and are inserted in a rack tube to give the necessary adjustment of distance from the slide. The series of powers, part of which have had their optical arrangements worked out on the screen expressly for this instrument by Messrs. Newton & Co., and the remainder of which have been selected by Mr. Wright from scores of lenses by exhaustive trials, are of the highest class, giving a flat image with full illumination and sharp definition to the edge of the field.

All the manipulations are absolutely simple and easy in the dark. The objectives are fitted to the R.M.S. Standard screw, and the body of the instrument is fitted to receive any achromatic condenser or other apparatus fitted to the standard 1½ in. sub-stage gauge. The apparatus may certainly be classed among the best-constructed and most ingeniously-conceived inventions of the day, and will doubtless be very extensively employed. It has already received eulogiums from many competent judges.

Miscellanea.

AMERICAN LIGHTHOUSES.—The United States Lighthouse Board has approved plans for a compressed gas-lighted beacon at Romer's Shoals, New York, for which 25,000 dols. were appropriated by Congress last session. The beacon will be a skeleton iron structure, 41 feet above low water.

REMARKABLE BICYCLE RIDE.—Mr. E. Oxborrow, of the Æolus B.C., has just accomplished an extraordinary day's journey on a "Facile" safety bicycle. Starting from Hitchin at midnight on Aug. 25, he rode, *viâ* Peterborough, Long Sutton, Lynn, and Swaffham, to Norwich and back as far as Biggleswade, which he reached at 11.58 p.m., on the 26th, having thus covered a distance of 263 miles in the twenty-four hours. This is within three and a-half miles of the present record, which was made last year by Mr. J. H. Adams, who also rode a "Facile."

A REMARKABLE CASE.—Dr. Strumpell, of Leipzig, had under his care, a few years ago, a young man who had suffered from a disease of the brain which had destroyed sensibility to touch over the whole body, and also the functions of one eye and of one ear. When this youth kept open his remaining eye and ear, he remained perfectly wide awake, conscious, and intelligent. But when his still efficient eye and ear were carefully closed, he immediately became unconscious—in fact, he could only remain awake by keeping that eye and ear open. This case is suggestive. It appears clear that the functions of the mind are dependent absolutely upon the reception of energy from the sensory perceptive areas.—*Newcastle Weekly Chronicle.*

AMERICAN ANTHRACITE.—The second geological survey of Pennsylvania has just published advance copies of its report on the anthracite coal region, giving some interesting information as to the production and shipments of 1884. During that year there were 377 producing collieries. The total shipments in 1884 were 30,718,293 tons, and the total production 32,641,499 tons, or about 1,300,000 tons less than in 1883. More than half the total product came from the Lackawanna and Wyoming coalfields, while the Pottsville coalfield, which, up to 1857, produced more than half the anthracite coal sent to market, yielded in 1884 less than 10 per cent. of the whole.

SCIENCE LECTURES IN LONDON.—The series of weekly lectures on popular science will shortly be recommenced at the Royal Victoria Hall (late the Victoria Theatre—more usually styled the "Vic") in the Waterloo-bridge-road. These lectures are given on Tuesday evenings, and while the charges for admission are little more than nominal, the committee have been able to secure the kind co-operation in their good work of the leading men of science of the day. Wisely appealing to as many of the senses as possible, nearly all the lectures are well illustrated by the aid of a fine oxy-hydrogen lantern. The lecture on Sept. 29 will be given by our contributor, Mr. W. Jerome Harrison, F.G.S., on "Stone Tools and the Men who used them," and will be illustrated by a fine series of slides.

SUICIDE IN NATURE.—Early in December, 1879, an apparent epidemic of suicide attacked the herrings and sprats in Deal Roads, and they rushed ashore in such myriads at Walmer that the fishermen got tired of carting them off, and they were left on the beach for all who cared to help themselves. Nature seems now and then to put bounds to over-population, but if this be the case, no herring famines need be feared, for economical Nature would never have played into the hands of the fishermen who are always at war with her. Such wholesale suicides occur among other forms of animal life. In Africa regiments of ants have been seen deliberately marching into streams, where they were immediately devoured by fish. Rats have migrated in myriads, stopping nowhere neither day nor night, and have been preyed upon by both large birds and beasts of prey. In the Seychelles some years ago several hundred turtles conspired to die together on the island in front of the harbour, and carried out their decision. Were they the victims of hydrophobia, delirium tremens, or some other disease? Even the gay and sprightly butterfly has been known to migrate in immense clouds from the land straight out to sea, without the remotest chance of ever reaching another shore. What could be the reason for such a suicidal act?—*From "The Sea"* for September.

ERRATA.—"Correlation of the two common chords of music, &c." In my letter 1899 on this subject there are a clerical error and a misprint which I shall be glad to be allowed to correct. In the series of ratios of the diatonic scale I have inadvertently written $\frac{1}{2}$ instead of $\frac{8}{9}$. Lower down will be found the word "imitated." It should be "instated."—FRED. J. JACKSON.

XUM

"Let knowledge grow from more to more."—Alfred Tennyson.

Only a small proportion of Letters received can possibly be inserted. Correspondents must not be offended, therefore, should their letters not appear.

All Editorial communications should be addressed to the Editor of Knowledge; *all Business communications to the* Publishers, *at the Office, 74, Great Queen-street, W.C.* If this is not attended to, delays arise for which the Editor is not responsible.

The Editor is not responsible for the opinions of correspondents.

All Remittances, Cheques, and Post-Office Orders should be made payable to Messrs. Wyman & Sons.

No communications are answered by post, even though stamped and directed envelope be enclosed.

FINDING THE WAY AT SEA.

[1900]—Mr. R. A. Proctor, in his interesting article on "Finding the Way at Sea," in Knowledge No. 199, Aug. 21, 1885, alludes to an important danger in steering a ship—viz., that her compass may seriously deviate on account of the presence of iron in her cargo. In the *St. James's Gazette* of the same date an instance is quoted in which the man at the helm found that he was unable to keep his vessel in her course, and on an investigation being made the irregularity of the compass which caused this difficulty was traced to a passenger's umbrella, which was found to act as a very powerful magnet, and, consequently, diverted the needle.

In both these cases the cause of the variation is accounted for, but I hope Mr. Proctor, or some of his correspondents, will be able to give the *raison d'être* for the following incident, which occurred some years ago, when I went, one of a party of four or five persons, from Algiers to Tunis by sea, on a steamer belonging to a French Company:—One afternoon, about 4 p.m., we were steaming leisurely along the North African coast, being at no time very far from the shore, only keeping out a sufficient distance to avoid the various headlands which jut out into the sea. It was towards the latter half of the month of April; we had a deep blue sky and lovely sunshine over head; the sea was as unruffled as a lake. As we passed a point where some of the higher ranges of the Atlas mountains were visible, the aspect of the heavens in that direction was black and threatening. Presently we heard distant thunder, and saw vivid flashes of lightning over these ranges, but our own surroundings remained as before.

Shortly after, the captain came round to all the passengers in turn, requesting the loan of any compasses which they might possess, for he said his own (and he had several) had all gone wrong. Our party between them produced two or three, and others were also given to the officer by various passengers; but they all exhibited the same unwonted variation, and continued to do so as long as the storm lasted, or until we got out of its influence, when they returned to their normal condition. The captain said afterwards that it was most fortunate that this circumstance had happened by daylight, for had it been dark he must have laid to, as he could not have been sure of his course. Cosmopolitan.

THE PHILOSOPHY OF CLOTHING.

[1901]—In his fifteenth article on this subject in Knowledge (No. 199, August 21, 1885), Mr. Mattieu Williams speaks of the desirability that down should be more generally used than at present, and of its great value in the manufacture of clothing. He suggests the possibility of "carding and weaving the filaments of feather down, such as common duck and goose down, swan's down," &c. This is by no means a lost art, for in Kashmir and in Ladakh, or Western Tibet, which are countries adjoining Central Asia (whence our remote ancestors probably came), a material is woven called pushmina, made from the winter under-coat of various animals, chiefly that of the goat, the bara-singh (a kind of red deer), and the ibex; the dog also has it in those regions. In texture, pushmina resembles the downy portion of the feathers of a bird, its staple is short, perhaps not more than an inch to an inch and a-half in length (rather less than more). The name of this down is pushm, it is made into cloth of the finest kind, and is exquisitely soft, pleasant wear, if it be the genuine article, and no *Liberties* have been taken with it; the real superior article is not easily obtainable in this country. There is one kind, which I only saw once in Ladakh; it is extremely rare, for the down of which it is made (that of the ibex) is only collected by the native sportsmen in very small quantities, and at very great elevations. When the snows begin to melt, and this animal finds its winter coat uncomfortable, it rubs itself against the rocks; the shikaris (hunters) carefully preserve every morsel they find, but it may be years before they can obtain a sufficient quantity of this down to weave even a puggree, or scarf to wind round the hat, turban fashion; the puggree would probably require to be about four or five yards long, and ten inches or so in width. The pushm of the ibex is incomparably finer than that of any other animal. It would be worth inquiry, and is perhaps not impossible, that many animals in the north of Norway and Sweden and in Russia (at all events, those which live within the Arctic circle), acquire this pushm to enable them to withstand the rigours of their winter climate, in which case we need not send to Asia for it, provided we could ensure that such down was not adulterated with other substances. It would then be worth while to try and catch a few Kashmiris and get them to teach us the art of preparing the fibre. Cosmopolitan.

GREASY DUCKS.

[1902]—I am not disposed to carry the discussion on this subject any further, as it is simply a question of fact to be settled by observation, not by writing. I simply deny, positively and dogmatically, that any of the feathers of these animals are greasy, and appeal to ducks rather than Huxleys for proof. "G. A." quotes great authorities against me; so much the worse for the authorities, and so much the better for me; he simply proves that, *re* duck's grease, I am a better naturalist than they; that on this mighty subject I have made and proclaimed a great discovery. I have half converted (the lower half) "G. A." himself. August 7th, p. 123, he describes the greasing process as carried "over all parts of the body feathers." August 28th, p. 187, he gives up the greasy breast —*i.e.*, the part immersed in water, and most in need of the alleged waterproofing—and falls back upon the root of the tail as the greased part, this being always out of water, even when the duck tips up, with head immersed, in search of food below.

W. Mattieu Williams.

STARLIGHT.

[1903]—I am delighted to find that my idea that the light of a moonless night does not come from the stars is established. (At the same time it is surely rather hard on Mr. Madge to say it exists only in *his* imagination, for it is the universal belief among men —saving those better informed—"a fine *starlight* night.")

I believe that our air is luminous for this reason:—Given two equally cloudy skies, one with high barometer—therefore high clouds, the other with low clouds, the twilight (daylight also, probably, but not so obviously) will be much longer and brighter in the first case than in the second. Now, the clouds cutting off all light, *ab extra*, it appears that the thicker the earthward stratum of air the greater the illumination; hence the air must be luminous.

The keen observer of "Wild Life in a Southern County" writes (xlii.):—"I have myself been often much interested in the remarkable difference of the degree of darkness when there has been no moon. There are nights when, although the sky be clear of visible cloud and the stars are shining, it is, in familiar phrase, "as black as pitch." The sky itself is black between the stars, and they do not seem to give the slightest illumination. On the other hand, there are nights without a moon, when it is (though winter time) quite light. Hedges and trees are plainly visible; the road is light, and anything approaching can be seen at some distance; and this occasionally happens though the sky be partly clouded. So that the character of the night seems not to depend entirely upon the moon or stars. The shepherds on the hills say that now and then there comes an intense blackness at night which frightens the sheep and makes them leap the hurdles."

In all my life there is one particular night which stands out as the blackest—June 4, 1854. I had to walk home through Bedford, and if I had not known it well enough to do it blindfolded I should really have been obliged to grope my way along the streets. This, too, was in the period of "no real night."

It may be said, "If the air is luminous why is not a shut-up room at night as light as when the shutters are left open?" Probably because the volume of luminous matter enclosed is so small.

The popular proverb says, "The darkest hour is just before dawn." If that is true one might conclude that the luminosity of the air is due to the past sunshine, and is hourly decreasing. This might be decided by observation within the Arctic circle, were it not that the moon and the aurora would introduce new elements into the problem. Hallyards.

REMARKABLE SKY.

[1904]—August 3, going out between ten and eleven, p.m., I noticed parallel banks of nimbus in the north, and general cloud elsewhere. In a short time these banks spread over the whole sky, and then an arrangement supervened which is unique in my memory. The nimbus became reticulated; one saw cumulus through meshes of jet-black nimbus. The sky looked like the eye of an insect under the microscope; or an immense net thrown over the world. It gave me the impression of being translated to some other planet, with different conditions prevailing. Near the zenith was an intensely-bright white opening, apparently cloud, but I could see stars, and even small ones, through it. I thought at first this cloud was moon-lit, but there were no signs of the moon rising for long after (she had just entered her last quarter). I inclined to think the apparent illumination was only an effect of contrast; the nimbi being so intensely black. They then broke up into irregular, menacing masses; finally, they united into the semblance of a giant balloon—really balloon-shaped, with a depression in one shoulder, and stretching from E. to W. This was really awe-inspiring; not only did it suggest a messenger from another world, but I feared it was going to swamp me completely in a water-spout.

Parturient montes, however, no rain fell that night or the morrow.

I cast about for a cause for this singular reticulation, and think I found it. The upper air was no doubt hot, the weather having been so for long; nimbus for some reason spread over my region; there blew a chill northerly breeze, and this would create a vertical upward current into the hotter upper air. This, meeting the stratum of nimbus, would punch holes in it where it was weakest, hence the network.

HALLYARDS.

[More probably, I should imagine, the cause was akin to that which produces reticulate waves—a wind producing a series of longitudinal atmospheric undulations followed by a cross wind producing a similar series at right angles to the former.—R. P.]

EVOLUTION.

[1905]—"It would be disappointing to those who believe in a natural evolution towards perfection to learn from Mr. Colquhoun that the modern Shans are quite as low in the scale of civilization as their ancestors were four thousand years ago. Some of these who have deserted the mountains for the plains of Siam, have shown their capability for advance by adopting the culture of the Siamese; but those who still remain in the hills are neither better nor worse than the Mon, Knei, and others, from whom they sprung."—*Saturday Review*, June 13, 1885.

Is it not well to bring to the front all instances, like the above, which *run counter* to received and fashionable theories?

HALLYARDS.

[But who that knows anything of the doctrine of evolution does or can believe in "a natural evolution towards perfection." That writer in the *Saturday Review* has evidently not been a reader of works on natural selection. The facts recorded do not in the slightest degree run counter to the theory of evolution by natural selection.—R. P.]

[1906]—Having in the last issue of KNOWLEDGE received, both editorially and conductorily, a somewhat forcible rebuff, the sort of lightning stroke of annihilation which I, alas! too fondly, used many a time and oft to admire and applaud with malicious merriment, when falling on the devoted head of another, will you permit me to say a word in vindication of my personal humility and of my respectful veneration of Darwin, and likewise to remove misapprehension by a slight simplification of what I have already said?

If my letter appeared to show an overweening confidence, it was the necessary consequence of having to attack, so to speak, in half-a-dozen words, a structure of such lengthened and masterly labour. If I treated it unduly in a spirit of light-hearted levity, it was owing to a desire to be readable, and from no want of reverence for Darwin, with regard to whom I yield to none in respect.

As to the subject matter of my letter, I wished to bring out, by means of a rather familiar example (which, homely as it is, ought certainly to be met, for it is sufficiently typical), the fact that small variations cannot be at all effective in the struggle for existence. Through the whole of "The Origin of the Species," which contains some twenty-four references to observed variation (chiefly, of course, under domestication), there are only two, and these doubtful, where natural selection, as apart from intelligent breeding, has seemed to perpetuate a slight variation. These are the growth in animals of an instinctive fear of man, and the development of small wing-bones and large leg-bones in the domestic duck. Now, Darwin distinctly states, "Any being if it vary *however slightly* in any manner profitable to itself under the complex and sometimes varying conditions of existence will have a better chance of surviving and will thus be naturally selected." This, as italicised, is what I have tried to deny. Again, "Natural selection can act only by the preservation and accumulation of *infinitesimally small* inherited modifications." "The *slightest* advantage in one being at any age or during any season over those with which it comes into competition, or better adaptation *in however slight a degree* to the surrounding physical conditions will turn the balance."

I quote these in full because it is so astonishing that, in the face of these very definite statements, he should admit a very real difficulty "in understanding the origin of simple parts of which the importance does not seem sufficient to cause the preservation of successively varying individuals." He does not mean such developments as are useless, but those of minor advantage (*e.g.*, giraffe's tail as fly-flapper). Surely this is inconsistent. He goes on to show, not as one would expect from the above quotations, that such unimportant variations, because even infinitesimally advantageous, are still under the control of Natural Selection—that consideration is for the present abandoned—but that such slight variations may perhaps affect an organ of more importance than at first sight appears. Infinitesimal variations are postulated, and then, as soon as we are brought face to face with the fact that they are so small, they are silently abandoned.

There is nothing else in my letter that I think it worth while to contend for, though I should like to reiterate the necessity for caution in accepting theories so agreeable to the time. To me, our Editor and Conductor, in both endeavouring to explain my letter by assuming with such calmness that I had not read a dozen pages of Darwin's works, are only another sad instance of the danger of falling into false hypotheses.

P. J. BEVERIDGE.

[I said had "not read *with attention* ten pages," &c. It is curious that reading Mr. Beveridge's letter in proof, before I had noticed the Editorial note, I thought of adding almost word for word what the acting Editor had already added. What I actually said may be regarded as a comment in support of the Editorial note. I still consider that Mr. Beveridge has not caught the true meaning of Darwinism,—indeed so much is obvious. He says "slight variations are totally ineffective," and illustrates by an example utterly unlike those considered by Darwin, which are such as leave a balance of advantage in the struggle for life under complex conditions. Mr. Beveridge does not pretend to show that the length of his moustache would affect him unfavourably in this sense. Even his slower feeding might be advantageous, while the protection to him in breathing might be a matter of much greater importance. If men had to feed in haste, and the long-moustached could not get a sufficient supply, then—unless length of moustache gave some equal or greater counterbalancing advantage—the case considered by Darwin would arise. Long-moustached persons would be weighted in the struggle for life, and in the long run the small difference would tell. As for Mr. Beveridge's present letter it confirms my opinion that he has missed Darwin's meaning. Darwin certainly has nowhere abandoned, silently or otherwise, the belief that slight variations, when they affect, in however slight a degree, the *balance* of advantage in the struggle for life, will have a better chance of surviving, and will therefore in the long run be naturally selected. In the last paragraph of letter 1879 Mr. Beveridge showed that he had utterly failed to follow the reasoning which led Darwin to reject the doctrines of Lamarck and others in regard to evolution. He should compare the editions of Lyell's "Principles of Geology" preceding Darwin's work with those which followed that work, if he would find a striking illustration of the contrast which exists between the theory of natural selection and all former attempts to establish a doctrine of biological evolution.—R. P.]

SKIN-CASTING OF SNAKES.

[1907]—In reference to Dr. Hutchinson's letter (1886) on "The Skin-casting of Snakes," may I be allowed to say that last year I kept snakes, and had good opportunities of observing their skin-casting practices? One of them, indeed, "exuviated" no less than three times within a period of five months, and two others certainly twice. As I kept my snakes well supplied with food, possibly a somewhat rapid enlargement of their graceful persons caused them to outgrow their garments more quickly than usual; for I was much surprised at the frequency of their demand for new clothes. More than once I had the privilege of assisting at the ceremonial, not without earning, I trust, the gratitude of the snake. On such occasions, noticing that the skin around the jaws had cracked, and that the head was emerging—the two flaps of the skin turning backwards—I took the snake out of its box, and encouraged it to crawl through a sort of manual or digital tunnel, when off came the skin, neatly turning inside out as the reptile crept through my hand—the peeling process ending with the tail. In fact, the whole affair much resembled the drawing off of a stocking by turning it

XUM

inside out from the knee downward. In one instance the snake left the point of its tail in the old skin, owing to a pinch which it had uncomplainingly received a week or two before through my own carelessness. The wound soon healed, but a slight terminal bluntness afterwards distinguished the tail of that snake. I may mention that this snake (whom I named "Sarah"), though perfectly tame and gentle, and possessed of attractive and endearing manners, had a voracious appetite, and once or twice actually snatched a frog out of my hand. One day, after swallowing several efts for breakfast, she greedily bolted two large frogs. Sarah had literary tastes; or, perhaps, a turn for practical joking, for she would coil herself amongst the books on my writing-table, and then, when I began to write, unwind herself and crawl over my MS., seeming to take a pleasure in smearing and blurring the sheet. Another snake, whom I called "Tiger," was a splendid fellow, adorned with a bright orange ring and spots. When he first came into my possession he was very fierce, hissing and striking at my hand each time I approached him, though he never bit me; but with frequent handling and gentle treatment he became a very courteous snake—as tractable as Sarah, though perhaps not personally attached to me so decidedly as that graceful, but not brightly-coloured, lady. A foreign snake (with black-and-silver longitudinal stripes, and traces of a ring round its neck), which I bought at a shop in Seven Dials, after a season of comparative gentleness, became so irritable and savage (from disease—"boils and blains")—that I was obliged to kill it. Besides several large English Colubers, and the foreigners (also of the Colubrine family), I had for a time two Coronellas (from Suffolk); but as these refused to eat efts, and sand lizards (their proper food) were difficult to obtain, I gave them to the Zoological Society. These snakes were not particularly endearing in their ways, and were in the habit of biting my fingers, though without drawing blood. They displayed considerable "constricting" and climbing powers, needing very little help to enable them to creep up the perpendicular trunk of a large tree. I noticed that the first symptoms of approaching exuviation was a general dulling of the colour of the snake—the eyes becoming dim, then of an almost turquoise blue, the reptile, too, apparently losing its cheerfulness and getting into a depressed state. After some days the colour would brighten somewhat, the new skin perhaps beginning to show through the old, the eyes growing clearer, and the countenance, too, assuming a more hopeful expression. After a further interval, the loosening of the skin about the mouth would take place, and then, in a short time, by creeping amongst stones, bits of wood, &c., the creature would turn back and strip off its old covering, and emerge in brighter and more beautiful apparel, and, evidently, more cheerful spirits. The skins, when off, are quite colourless, and show no holes where the eyes were, but curious spectacle-like projections. The cast-off coats of my English snakes measured from 41 to 43 or 44 inches in length. I forgot to notice that Tiger and Sarah occasionally amused themselves (and me, too) by pulling off my spectacles. They accomplished this by creeping between my face and the glasses and so pushing them off. I may also mention that, years ago, I was savagely bitten by a very large English snake, the common "Coluber natrix Tropidonotus." The creature's teeth were certainly very sharp, and there was some amount of bleeding, but, beyond a little smarting of the wound, of course, no bad consequences ensued. C. H.

Aug. 25, 1885.

ULEY BURY.

[1908]—On a walk through Gloucestershire this week, I happened to pass over Uley Bury, which I see is mentioned in your paper of yesterday, so venture to forward you the following notes, though, like most tourists' notes, they are worth but little. The Bury is well separated from, and commands the surrounding hills. The summit is a tableland, partly under plough cultivation. The sides are very steep: on their edge is an earth parapet and single ditch. There are roadways on the south-west face and at the north angle, both fortified by out-works; that at the north angle has had special attention. This gate faces the road over Crawley Hill to Nailsworth, and is the easier way into the camp. As this road is a ridge-way, it is probably on a very early road track. The defences here are two parapets, their front elevation being like segments of two concentric circles; the north corner of the front and smaller one has been cut deeply into for stone to mend the roads. It does not seem to have been worked of late, and it is to be hoped it will not be again (for stone abounds here at the very surface), as the contour of this defence would soon be destroyed. The enclosure is about half-a-mile long on its longest side, and rather more than a quarter on its opposite side, its breadth being about one furlong. From the slightness of the parapet it seems to have been formed in pre-Roman times before the British found it necessary to make such formidable triple earthworks like those on Caer Caradoc and the Herefordshire Beacon. From its large size one would think that a considerable population from the surrounding fertile vales expected to find a refuge there in time of war. Cultivation has obliterated any signs of hut circles or barrows, if any existed, which make the fortified town on Midsummmer Hill so interesting.

Perhaps some one who has taken an interest in this hill will send you a more detailed account of it than these vague notes.

Aug. 22, 1885. E. Cook.

VERSATILITY.

[1909]—I fear that "Hallyards" has an uneasy suspicion about versatility in general, and his own peculiar form of it in particular. Surely I praised it highly enough! And he replies that I "insist on the man of one book." I have not "insisted" on him, but I infinitely prefer him to the "man of no books at all," who forms his own opinions (as the clever boy made the chest of drawers) "all out of his own head," who publishes by the dozen crude ideas on subjects to which the greatest intellects have devoted years of patient study. The man of no books at all is ever the most ready to "argue the point" on, say, the principles of evolution, before he even knows what evolution teaches (see letter 1872); he hears of the law of diffusion of gases, and instantly sees that it is "an influence counteracting gravitation as a universal law;" he solves the "problem of three bodies" off-hand, thus: one of them revolves round both the others, which place themselves in the foci of its ellipse—each, no doubt, flattering itself that it is the centre (I beg pardon, I should say "focus") of attraction. No wonder that after such a brilliant "happy thought" as this, the man of no books at all should "leave the dynamical question." He is prudent, for something astonishing must undoubtedly soon happen to this pair of jealous suns, and their eccentric satellite. Then, again, it is to the man of no books at all that we are indebted for such gems of knowledge as that the earth is flat, that the ratio of the circumference of a circle to the diameter is as 3¼ to 1, and generally that trained men of science are hopelessly wrong about everything.

W.

["W." will excuse the omission of a part of his letter. Correspondents object to discussion not leading to any increase of knowledge, and "Hallyards" thinks himself unfairly treated if not allowed full swing in reply to all that may be said of him. In passing I note that the saying "Beware of the man of one book" had quite the opposite meaning to that imagined by "Hallyards." It was an expression of respect not of contempt. Beware of him for he will probably be strong and sure.—R. P.]

RELIGION OF THE ANCIENT MEXICANS.

[1910]—In reply to "Zeteo," letter 1877, he will find in Logan Mitchell's "Mythology Revealed," p. 101, a short *résumé* of the facts mentioned. This work was published first by Trübner, but there is a cheap reprint of it published at 63, Fleet-street. Full particulars can be obtained in "Narratives of the Rites and Laws of the Yncas," translated by Clements R. Markham, also "Commentarios Reales de los Yncas," by the Ynca Garcilasso de la Vega, Prescott's "History of the Conquest of Mexico," Prescott's "History of the Conquest of Peru," "Legendas Mejicanos, mentos y baladas del Norte de Europa," by F. M. Barcena, all published by Trübner. There are also the works by Acosta, Las Casas, Herrera, Sahagun, &c., containing full accounts.

The Spaniards were so much struck by the remarkable resemblance of these rites and doctrines with their own religion, that their priests attributed them to the machinations of the devil, and, for easily divined reasons, did all they could to prevent the facts from becoming generally known.

The principal features are: they represent their God under a trinity. Their sacred emblem is the cross. One of their trinity was crucified on a mountain between two thieves. Resurrection after three days. Ascension through the clouds. Expected return of their Saviour. They had the rite of baptism. The immaculate conception of a Virgin by the son of their God, &c. In short, all the leading features of Christianity and Buddhism, showing the common origin of "solar worship." F. W. H.

[1911] "Zeteo" [1877] will find the information he requires in "Hibbert Lectures for 1884. Religions of Mexico and Peru." By Albert Neville. C. Cridland, M.D.

THOUGHT TRANSMISSION.

[1912]—I read with interest the discussion appearing in your paper on the above subject, and imagine the following will interest those interested. The other day I was thinking on a subject in which one of my comrades at another station was the principal. I went to instrument and "shook" instead of "calling in code." Immediately he answered. I asked a question, and then asked how

he came to answer correctly. "Why," says he, "I was thinking of the same thing myself, and walked to instrument, having an idea it was you, and you wanted my attention." Was it mere coincidence?

R. T. C.

INDIAN WASPS AND HORNETS.

[1913]—In my paper on Termites I did not include these Hymenoptera among our pests, because we are so familiar with the first, and generally find them so harmless, that we don't care for them.

On the other hand, our contact with the terrible hornet is fortunately so rare that we cannot class them as *general* pests. When that contact does occur, an *annus notabilis* is added to the calendar of the individual, if he survives.

Some years ago there was a controversy as to whether cell-building in hives and nests was centripetal or centrifugal. In a ruinous outhouse at Peshawar, I was able, in 1879, to satisfy myself of the latter; dependent from the roof, by a slender stalk, was the *solitary* paper cell of a female wasp, and she was proudly and tenderly overhauling its proportions with her anxious antennæ; while from the base of each side of the hexagon was already sprouting the germ of another cell, and so, under the fostering care of that lonely and indefatigable architect, the fabric reached maturity by diverging centrifugally from that focal cell.

The familiar English wasp is not met with out here; its place being taken by the yellow hornet, which thoroughly domesticates itself in our houses, and very very rarely assails, and then only in self-defence; the verandahs, bath, and storerooms are favourite attachments for the paper nests, which, when different families combine, become very large structures. During the breeding-season three or four males will secure points of vantage in your study, especially in the bookcases, and there each individual will take up a calling-spot; resting on his hind legs, with his fore elevated mantis-like, he keeps up a constant buzz, and surveys the scene, his antennæ moving anxiously in all directions. Woe betide the rival male who dares to invade that calling-ground!

But hush! here is a counter-buzz, recognised as female, and our beau is frantic with blandishments; he bows, stands on his hind legs, sings entreatingly—

"Won't you come in my pretty, pretty maid?"

while the lady approaches and retreats in true flirtish style; having sufficiently agonised the beau, she settles, and immediately becomes the centre of his most agonising attentions; he licks her all over, kisses her with his antennæ, smoothes her wings, and ends by fertilising her. His function being completed, he dies. She re-appears as that anxious and expectant mother in the Peshawar outhouse, and prepares her cradle for approaching maternity, the material being apparently semi-digested woody fibre. As each cell is completed, she enters backwards, and lays an egg, then fills up the nest with propolis, and arches it over with the same paper of which the cell is constructed.

This cell-building and egg-laying goes on until her ovaries are exhausted, and then she dies; so that each egg will pass into a larva, and that into a perfect insect without acquaintance or contact in any shape with the parent insects. Other wasps build their nests of mud, which you see them collecting in pellets from any spot where water lies—*e.g.*, the drain of your bathroom, or the neighbourhood of a well. These nests are only cylindrical tubes of various calibres, and with no attempt at geometric form. Some of these structures are plastered over with gum-arabic by their architects—why, I know not.

A very troublesome wasp affects ready-made holes of every description. In some localities, when the mosquito-curtain season is over, you will invariably find the female screws in the bed-posts utilised by these nuisances, the orifices being carefully plastered over and then whitewashed; or you may find the lock of a box or drawer which hasn't been opened for a week similarly stuffed up and whitewashed.

The final touch of whitewash is very wonderful, and is probably stolen from the walls. I say wonderful, because it is really a check upon the too rapid propagation of that particular species, for wherever you see the *white* mark, it is almost invariably attacked by master or servant, and the laboriously-collected contents dispersed.

But *the* most wonderful fact connected with these mud-building wasps remains to be told. We have seen that the common yellow wasp stores up propolis in her paper cells; not so the mason wasps, who fill up their nest-tubes, each with a different species of caterpillar or spider, each victim being in some wonderful manner anæsthetised and embalmed so as to afford *living* food to the enlarging grub.

Here is a case in point which we can watch as we write. The sudden appearance of a black, or very dark-brown, caterpillar-laden wasp draws our attention to its nest in the verandah, a series of mud cells opening upwards. As she alights we notice that she is on the back of the anæsthetised caterpillar, holding it in the grasp of all her legs. Quickly she manages to get the tail end of the caterpillar (in this case one of those green loop-walking larvæ of some ourapteryx moth) into the cell, emitting the while a curious hum, not alar, but thoracic or vocal, and then presses it in with her feet.

Flying away, she returns with a second loop-walker, which is similarly treated, then with a third and fourth, and the cell is full. It is then carefully plastered over, and a fresh one is begun. Eight to ten cells are thus laboriously completed and provisioned, and then that anxious and laborious mother yields up her life.

Some wasps bring brown, others green, spiders to their cells. In fact, each species sticks to its own caterpillar or spider—*i.e.*, a spider wasp will never bring in a caterpillar, and *vice versâ*. This I have established as an absolute fact.

Having reached thus far, let us review our position. On the one hand, the common yellow wasp builds her nest of paper, *always* of the same colour (dull white) and material, deposits in each cell an egg, fills up with propolis, and dies. Her offspring (taking one individual) has never seen its mother, never before seen the cell from which it emerges, can learn nothing from the outer world as to the material of its cell, nature of its food, or whence both are procured; yet it has no difficulty in procuring the proper kind of food, nor, when the fit time arrives, has it any difficulty in procuring the proper paper for its nest of cells, nor in moulding the latter with mathematical precision equal to that of its parent.

In like manner, on the other hand, the larvæ in the mud cell of this wasp-nest has never seen its parent, and beyond gorging on them in the darkness of its prison, has had no knowledge whatever, beyond their taste, of the green loop-walkers its mother has so carefully collected for it. Yet, when its time comes, it constructs a similar mud-nest to that of its parent, and stuffs it with similar larvæ, and no other.

Now, who taught these wasps the wonderful machinery of their cells, the astonishing processes of selecting particular kinds of animal food for each kind of cell, and of keeping such food alive, but anæsthetized, as long as it may be required?

The popular reply will be—instinct; but that tells us nothing. Why should a new-born wasp be better able to provide for itself than, say, a puppy, or even a baby—animals far higher up in the scale than itself?

By what process of development or natural selection has this marvellous vespa been perfected? for, mark, while it was developing, the loop-walker was undeveloping, *i.e.*, it was having its legs re-arranged, head and tailward, and from moving gracefully and equally, it had to adopt painful and laborious contortions, and thus become the fit and only food of a particular wasp.

Now for the proper hornets. Two species of these terrible insects are commonly met with, but fortunately both are comparatively rare. The lesser hornet is 1½ in., and the greater 2 in. long; both species are brown, with a broad yellow band across the abdomen; both frequent unused buildings, tombs, and desolate ravines; the paper nests of the greater hornet are seen in the firs and cedars of the Himalaya, and many a traveller has jeopardised his life by foolishly firing at the nest. When attacked, your only chance of life is to sit down at once and cover yourself with a blanket. If you run, you are a dead man. Two cases in point. In the end of 1883, my friend, Surgeon-Major R. Jackson, A.M.D., in charge of the depôt up here was attacked by the lesser hornet; losing his presence of mind, he threw off his coat and ran for his life, which he lost in a few days by general blood-poisoning, having been stung from head to foot.

In 1861 I was showing the Arrah House to some friends, and, to get more light in one room, burst open a small window. In a moment I felt an agonising stab in my left temple, and just recognised my assailant as a greater hornet. Knowing what was coming, I made for home (about 250 yards distance) and ammonia. On reaching the threshold, I was paralysed, and couldn't move. Violent tremor succeeded, in the midst of which I was able to ask for a glass of port wine and swallow it. I was then laid on a couch, the tremor gradually ceased, and I fell asleep, awaking quite well, but in a profuse perspiration. A lump like half a pigeon's egg remained on my temple for some days.

Pachmari, July 11, 1885. R. F. HUTCHINSON, M.D.

ORGAN-PIPES.

[1914]—Of the Holmes organ in Battersea Palace I have read, "The biggest pipe, which weighs one ton, situated in the centre tower, is 38 ft. high, vibrating thirty-three times in a second, on sounding the lower C. The organ has five stops more than Haarlem, four more than Freiburg" (*Pall Mall Budget*, May 1, 1885).

I have always understood that the lowest pedal-pipe in an organ (thirty years ago) gave seventeen only to the second, and CCCC.

XUM

This would be two octaves below "the lower C"—*i.e.*, C below the bass lines.

I was lodging ten years ago at No. 1, Gloucester-terrace, Campden-hill. The rail passes nearly beneath the house, and there is an open part of the line behind the house. Sometimes—not after every train, but after few—I used to hear, after the clatter and roar of the train had quite died away, a faint sound, so horribly low that I was fain to stop my ears, fearing some injury. It occurred to me, on thinking, that this sound must be the note of the tunnel, considered as an organ-pipe. How many vibrations would that give? The rarity of the occurrence I conjectured to arise from its vibrating only when two trains chanced to pass each other exactly there.

Is there any probable determination of the musical value of the grunt of a very fat hog (not, of course, its value to an *impressario*, though I *have* heard a wild tale of an Admiral, who, lacking a band, and possessing a lot of pigs, had them ranged in the order of their pipes, and then played like musical glasses)? HALLYARDS.

ANT-ACIDS.

[1915]—I have just made a curious discovery. Writing in the fields, as usual, and over-run by smallish black ants, I smashed one in my fingers, being very fond of the smell of formic acid, found in them only (which shows how very human I am, for it is the proper sauce of man—the Maoris always stuffed long pig with ants before roasting). To my surprise, the smell was that of a first-rate fresh-cut lemon. I tried another; same result. I suppose hence that this species is full of *citric* instead of formic acid.

After a few minutes my fingers still have a lemony smell, but not so marked as at first. HALLYARDS.

MUNCHAUSEN REDIVIVUS.

[1916]—"A curious circumstance once happened to me at Pulney Loch. One of my sons threw a live mouse into it, when a large trout took the mouse down immediately. The boy told me what had happened; so I took my fishing-rod, which was leaning against the house, down to the loch, and put a fly on. At the very first cast I hooked a large trout, landed it, and laid it on the walk. In two seconds the mouse ran out of its mouth, and got into a hole in the wall before I could catch it."—W. Scrope's "Salmon-Fishing."

"It is a sign of narrow-mindedness to be incredulous," remarks the *Saturday*. There is in the story of Jonah nothing more intrinsically incredible than in this of the mouse. It teaches that a small mammal may live, without syncope, in the maw of a fish for a much longer time than it would live under water, or, at least, seem to live. Now, in 1879, a woman was recovered at Paris after a *twenty-four* hours' apparent death by drowning, through the incessant (and, one would say, insane) efforts of a doctor and a druggist. Given a man (of the old, tough type, now extinct) in the maw of a *mammal*, kept warm, resisting through his life the gastric juice, and receiving air as frequently as the mammal itself, and there is no impossibility in his maintaining this position for three times twenty-four hours. If he thus remained, resisting digestion, and probably not quiet, it is certain he would presently be vomited up, like any other tough morsel. A boa swallowed its blanket once, and it was rejected after (I think) a month, with all the nap digested off it. Experts were of opinion that, when the *Captain* foundered, asphyxia would not finally triumph before the end of twenty or thirty minutes. This is perhaps the saddest outcome of the "*æs triplex*" of the first mariner.

Captain Lloyd, in his "Field Sports of the North of Europe," says that eagles sometimes seize pike, and are dragged down and drowned by them, being unable to disengage themselves; but this acquisition of an imperial crest is by no means a feather in the pike's cap, for, of course, they can't swim with any speed, so handicapped (or unhandicapped), and are forced to skirt the surface. The eagle's skeleton, grown green with weed, thus flits o'er the waters of the lake, and is taken by the natives for the water-sprite—a harbinger of misfortune. A pike thus crowned was shot. Three cases are also given where an eagle stuck one talon into a root, and the other into a salmon, and, the fish darting off, was forthwith split from rump to beak. Note the immense strength of the bird's legs, which one would suppose would give before his body. A similar fate had nigh befallen me once. Stepping into a boat, the shore-foot got between two stones and the other entangled in a rowlock; had the forces at work been disproportioned, the result would have been dislocation of the hips or knees.

On entering one day the coffee-room of the Star and Garter, at Kew, I saw a cat with a canary in her mouth. I immediately went for the cat, who, strange to say, did not run, though two doors were open; put my thumb on her throat and delivered the bird, which an hour after was no worse. HALLYARDS.

LETTERS RECEIVED AND SHORT ANSWERS.

HALLYARDS. You are mistaken. There is usually more aqueous vapour in hot than in cold air. It is this capacity for receiving aqueous vapour which gives to hot air its drying power. As you point out, a committee of washerwomen would probably agree with you: but after you had studied the matter you would not agree with *them*. As to "the waters above the earth," I think you are probably right. People in old times found that water came down from the sky, and so—probably—supposed there was a reservoir up there.—Doubtless there were builders before the days of the pyramid. —I have not "stated as a fact that the moon is a dead world": I think it likely she is,—that is all.—With regard to corruptions of English, the subject is one which, if continued, would require that our correspondence columns should be left open. Now after the end of September there will be neither correspondence nor replies to letters received. Letters written by those who know may either appear in article form or be made the subject of paragraphs. But KNOWLEDGE will be a monthly sixpenny magazine of science, literature, and art.—Your letter about disagreeing Doctors will not "work the oracle": I have seen neither the book nor the review, and therefore cannot say what either Mr. Tait or the reviewer meant.—HUNDREDWEIGHT. "Farther from" was intended. See "Hallyards'" letter, and my "Gossip."—R. LEWINS. We must beg to be excused.—W. N. MACARTNEY. In my lecture on "The Sun," I usually dwell on the terrestrial effects of solar rays. Also in my lectures on "Life of Worlds" and "Origin of Solar System."—F. J. BURGOYNE. Your society's name would then be first on that list, which is as yet not opened.—C. MOON. Of course the difficulty is that most mathematicians do not know about the practical wants of seamen in such matters.—F. C. W. Begin with the "Origin of Species"—then the "Descent of Man."—R. S. TARR. Found nothing enclosed.—T. B. S. I differ from you, but I have no difference with you. When a man says, I differ with you, he does not mean I have a difference with you, but I differ from you. That a man may differ from another in height, colour, &c., gives no reason why he should not also differ from him in opinion.—BORDERER. You will find the lines in Anstey's translation of "Faust." As you say, they have been given in KNOWLEDGE, so space must not be used in repeating them among replies.—THOS. SMITH. When has man been regarded as *not* an animal? If you ask what difference there is between man and other animals, I point out that man speaks, reasons, and is moved by feelings and emotions either not experienced at all by other animals or not in the same degree. If a man is dumb, idiotic, and brutal, he is not in these respects unlike other animals—and I must go back to some such definition as Plato's, Man is a featherless biped, though even that was spoiled when a plucked fowl was thrust before Plato's pupils. In short, I expect you know as well as I can tell you in what man differs from other animals.

Our Whist Column.

By "Five of Clubs."

BRILLIANT BUT NEEDLESS STRATAGEMS.

MANY seem to imagine that the Whist games which appear in books and papers are necessarily examples of perfect play, especially when those engaged are good players. As a matter of fact this is far from being the case. Often in the exercise of his best judgment, but on the spur of the moment—for at Whist a player is not permitted to enter on prolonged calculations—even the finest player does what he sees afterward to have been ill-judged. The finest player makes the fewest mistakes,—that is all that can be said.

But apart from this we sometimes find that in games specially selected as examples of fine play,* the very stratagems by which it seems at a first view as though success had been forced, have been in reality unnecessary and even ill-advised. I know no better example of this than the game from Cavendish's treatise on Whist, to which I referred last week—a game published by him as a fine example of a double *coup*,—a *coup* in throwing the winning card, and then the *grand coup* in using a surplus trump to capture

* Cavendish, in his review of my "How to Play Whist," remarks on mistakes in some of the games. Representing actual play, they are of course not free from mistakes; but, after all, the worst fault he notes is that in some games the antepenultimate is played, in others not. He wants "uniformity above everything." But as some players do, while others do not, play by system, games honestly taken from real play cannot be absolutely uniform. It is a much more serious fault when, as in Pole, five games, and in many of the thirty-nine in Cavendish, bad play is selected for commendation. The game dealt with this week may be cited as an "awful example."

partner's trick,—in perfect unconsciousness that this brilliant play was unnecessary and dangerous. The game appears in my work, "How to Play Whist." But readers who have written asking me where it is unsound may take interest in studying it with the accompanying series of notes:—

THE HANDS.

B { H (*trumps*). A, Q, 10, 6. D. A, 3 / S. Q, 10, 8. C. A, K, Kn, 8. }

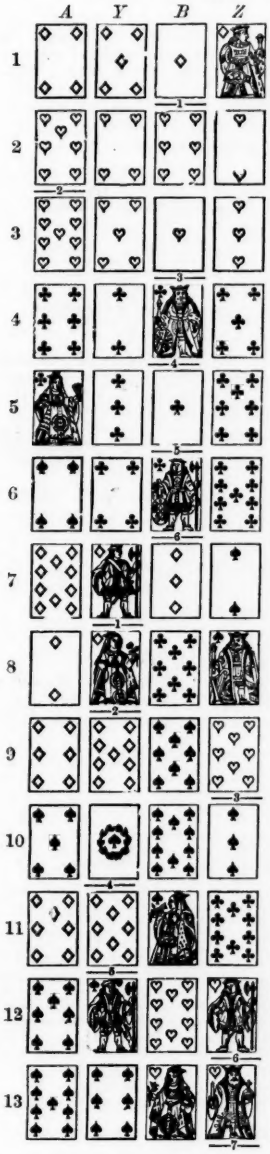
Y { H. (*trumps*) 5, 4. / S. A, Kn, 6. / D. Q, Kn, 9, 8, 5. / C. 4, 3, 2. }

B / Y Z / Tr. H. 8. / A leads.

K, Kn, 8, 3, 2. / K, 3, 2. / K. / 10, 9, 7, 5. } *Z*

A { H. (*trumps*) 9, 7. D. 10, 7, 6, 4, 2. / S. 9, 7, 5, 4. C. Q, 6 }

Score:—*A-B*, 4; *Y-Z*, 4.



NOTES ON THE PLAY.

NOTE.—Card underlined takes trick, and card next below leads next.

1. *A* leads penultimate of his long weak suit. As he has no re-entering cards, except the Queen of Clubs, which is very likely to fail him, the case falls under Pembridge's rule that a long weak suit should never be led from, without strength to bring the suit in. Especially is this bad at the score of "Four all." *A* sees that Queen lies with *Y*.

2. *B*, knowing that *Z* has no more diamonds, while he has but one left, can safely infer that *A* led from five. If so *Y* cannot be signalling, for the Two is the only card less than the Four (except the Three, which *B* holds). He sees, therefore, that *A* has the Two, probably, and three more. *Z* may be signalling from King, Queen; but this is less likely.

3. *A* can count nine trumps at least between *B* and *Z*. *Y* can hold no other trump but the Five. The return of the trump lead up to *Z*'s strength was therefore bad play, especially at the score. After trick 3, *B* and *Z* know that all the remaining trumps lie between them (note the trump card).

4, 5, 6, 7. *B* makes all possible Club play, and leads his partner's suit.

8. Here *Z* (Cavendish himself, the hero of the game) plays badly! It is obvious that if the enemy hold the Spade Ace the game is lost for *Y-Z*, let them play how they may; for *A-B* want but one trick to save and win the game. Therefore *Z* thould have played as if he saw the Ace in *Y*'s hand. He should have trumped his partner's winning Diamond, led his Spade King, and then the small Spade. He was sure of the game this way, because, after round seven, he knows *Y* holds the Diamond Nine. On this course *Z* would still have had the satisfaction of playing the *grand coup*. It would have done equally well, however, to play the quiet game, and discard the Club Ten.

9. Here again Cavendish played badly. By trumping his partner's winning Diamond he makes the game depend on partner's holding the next best Diamond. By discarding the Club, and leaving partner to go on with the next Diamond (losing or winning), *Z* would have held the game sure. But

11. Luckily *Y* holds the winning Diamond; so that,

12 and 13. Leading up to *Z*'s tenace, *Y-Z* win the trick and the game.

"Cavendish" was *Z* in the above game, and "Cavendish's" father, a good player of the old school, was *B*. In "Card Essays," Cavendish gives an amusing account of the game, and of his father's mixed feelings respecting the result—pride in his son's play, and annoyance at defeat. He assumes, however, that the *grand coup* at trick nine won the game. ("Of course it did, of course it did," his father grunted); while James Clay greatly admired the throwing away of Spade King Yet in reality, as Mogul long since showed, the game could have been more safely won without these brilliant strokes, and was endangered by the bad play which made them necessary.

Thus, suppose the game to have proceeded as far as trick six, *A-B* having won all six tricks, and wanting but one to win. At trick seven, Cavendish was perhaps right in omitting to ruff, as the game was lost if *Y* had not the winning Diamond, and withholding the ruff gave *Y* a chance of making the tenace if he had it. Still the game wonld have been easily won if *Z* had ruffed at trick seven, leading then Spade King, then small Spade, and leaving his partner, after winning with the Spade Ace, to play his winning Diamonds, which *B* dare not ruff. But at trick eight, both *Y* and *B* play badly. *Y* should have led the lowest of his head sequence of Diamonds, so that *Z* might ruff (for *Y* knows that unless *Z* holds three trumps the game is lost); and whether *Y* had led his lowest or highest Diamond *Z* should have ruffed. The tricks would then have fallen as follows.—

8th to *Z*'s small trump;
9th to *Z*'s Spade King;
10th to *Y*'s Spade Ace;
11th to *Y*'s Diamond Nine;
12th and 13th to *Z*'s major tenace in trumps;

all this is sure, because *A* certainly does not hold the Diamond Nine (see trick 7).

The game is also sure (apart, of course, from the enemy holding the Spade Ace, in which case nothing can save it) if *Z* discards Club Ten at trick 8. The tricks would then fall as follows:—

8th to *Y*'s Diamond Queen;
9th to *Z*'s small trump;
10th to *Z*'s Spade King;
11th to *Y*'s Spade Ace;
12th and 13th to *Z*'s major tenace in trumps.

This, undoubtedly, was *Z*'s proper course, after *Y* had erred by leading his Diamond Queen.

After throwing away the Spade King, and with it the only sure paths to victory, *Z* again played badly in the *grand coup* at trick 2. It was not only unnecessary; it endangered his game. If *Y* had not held the Diamond 8—and there was nothing to show he held it—the *grand coup* at trick 9 would have lost an otherwise sure game. The game should have closed as follows:—

	A.	*Y.*	*B.*	*Z.*
9.	D6	D9	S8	C10
10.	D7	D8	S10	H8
11.	S5	SA	SQ	S3
12.	S7	SKn	H10	HKn
13.	S9	S6	HQ	HK

If *Y* held the losing Diamond, the game would have been sure on this line, while lost on the other.

I believe that in a great number of cases where the *grand coup* is triumphantly played, the necessity for playing it has arisen from a previous blunder or two, as in this case. Of course, on every line by which the above game might have been won, the *grand coup* must have been played. But this is simply because *Y* holds all the best Diamonds after trick seven. For anything *Z* knew up to trick ten, the extra trump would capture the enemy's winning Diamond, not *Y*'s.

W. F.—A second solution is not usually regarded as rendering a problem "more interesting and instructive." There is, however, no second solution to the six-trick double-dummy problem. If *A* opens with C 10, taken by *B* (by the way *Y* must play the knave, or *B* can let *A*'s 10 take the trick and a third solution of the problem would come in), following with Spade 7, *Z* discarding Diamond 7, *Y* must not unguard his Diamond Knave; he unhesitatingly discards a Heart, for, as against the Ace and King, *Y*'s Hearts have no value. Now, let *B* lead as he may, *A-B* lose one trick, either to *Y*'s Club 8 or Diamond Knave.

Our Chess Column.

By MEPHISTO.

THE CENTRE GAMBIT.

ILLUSTRATIVE GAME No. 9.

WE give here a specimen of Black's defence of 4. P to KKt3 in this Opening. It will be remembered that we gave preference to the defence of 4. Kt to B3.

GAME PLAYED IN THE VIENNA TOURNAMENT OF 1882.

White. M. Tschigorin.	Black. Capt. Mackenzie.	White. M. Tschigorin.	Black. Capt. Mackenzie.
1. P to K4	P to K4	16. Kt to Q4 (*f*)	Q to Q2
2. P to Q4	P × P	17. Kt to Kt5	P to QB3
3. Q × P	Kt to QB3	18. Kt × P (ch)	K to B2
4. Q to K3	P to KKt3	19. R × Kt (*g*)	P × R
5. B to Q2	B to Kt2	20. B to Kt5	Q to K3 (*h*)
6. Kt to QB3	P to Q3 (*a*)	21. Q to B3 (ch)	K to Kt3 (*i*)
7. P to B4	KKt to K2	22. R to K sq. (*j*)	K × Kt (*k*)
8. Castles	B to K3	23. R × Q	P × R
9. Kt to B3	Q to Q2	24. Q to K3 (ch)	K to R sq. (*l*)
10. Kt to Q5 (*b*)	Castles QR	25. Q to R3 (ch)	Kt to R3
11. B to B3	B × B	26. B × Kt	P × B
12. Q × B	Kt to QKt sq. (*c*)	27. Q × P (ch)	K to Kt sq.
		28. Q to Kt6 (ch)	K to B sq.
13. Q to R3	B × Kt (*d*)	29. P to QKt4	KR to K sq.
14. P × B	Q to B4	30. Q to R7 (*m*)	P to K4
15. P to KKt3	Kt × P (*e*)	31. P to Kt5	Resigns

NOTES.

(*a*) Judging from experience gained in our previous games, we may assume that in all cases it is better for Black to play P to Q4 in this Opening, especially so when the Kt is on QB3, B on Q2, and Q on K3. For that purpose it would have been better for Black to play KKt to K2, followed by Castling, &c.

(*b*) A very good move, and showing the importance of occupying that square for Black. White makes room for his B on B3. Black cannot take the Kt, for after P × Kt the Black QKt is driven out of play.

(*c*) Black must make room for his Q, which is threatened with Kt to B6. To play B × Kt would give Black a very awkward game.

(*d*) Black should only have done this as a last resource. We do not see any very great objection against P to QR3, except that Black would not be able to dislodge the Kt any more by P to B3.

(*e*) An error which costs Black a piece. The Q only affords precarious protection to the Kt; there are several ways of proceeding, 16. Kt to Kt5 followed by B to R3 looks tempting, but Black would answer with 16. Kt × P.

(*f*) Well played, with a view to Kt to Kt5.

BLACK.

WHITE.

(*g*) A masterly conception, and, as will be seen, perfectly justified by the result.

(*h*) If Kt to B3, White plays Kt × Kt, followed by Q to R7 (ch), &c.

(*i*) It would be equally bad to play Kt to B3. 22. Kt × Kt, P × Kt. 23. Q × P (ch), &c.

(*j*) With the intention of playing Q to K4 (ch), followed by R to K3 and B3 (ch), &c.

(*k*) Taking his last chance.

(*l*) In reply to P to Kt3, White plays Q × P.

(*m*) An excellent move, Black now cannot move either Rook, and the P can go on to Queen.

EXPERIMENTING IN SERIOUS GAMES.

Two players sat down to play a game. The first player began with 1. P to K4, the second player then lapsed into deep thought and kept his opponent waiting for his reply. "Do you want to invent a new move?" asked A impatiently. "Yes!" replied B. "Then," responded A, "I wish you would do it at home!"

The same might be said to those players who jeopardise serious games by trying experiments in the Opening. Nevertheless it is a well-known fact that many players (and sometimes in spite of all resolutions to the contrary) will test some fancied line of attack or defence on important occasions, with the result that sometimes the experiment brings them anything but profit. Thus Steinitz lost several games through that reason in the London Tournament of 1883, as well as Zukertort, although the latter only kicked over the traces of Chess prudence after he had won the first prize. Winawer, more than any other player, creates for himself unnecessary difficulties by constant deviations from recognised modes of play. A brilliant exception from the general fate befalling players experimenting in the Openings, is shown us in the Chess career of Louis Paulsen. He contributed to, and extended our knowledge of, the Openings, more than any other living player, mainly by experimental play. He was, however, a successful player in his time. That may be owing to the fact that in the first instance he was purely an analytical player of very high order. Secondly, play in his time was by no means so hard and stubborn as it is now.

THE following remarkable calculation has been made by Herr Richard Schurig, Professor of Mathematics, and also a strong Chess player. He says: "The numbers of different positions which the thirty-two chessmen may form on a board of sixty-four squares, reaches the enormous sum of fifty-two figures, that is—
7,534,686,312,361,225,327,000,000,000,000,000,000,000,000,000,000,000,000
Some idea of the vastness of these figures may be formed from the following comparison. Suppose we have to deal with microbes of $\frac{1}{1000}$th of a millimètre in size, therefore a cubic millimètre would contain 1,000 millimètre of these microbes. A globe containing all the microbes represented by these fifty-two figures, would have a diameter of one-and-a-half times as large as the distance from the earth to the sun. But if, in addition to this, we also take into account the different positions which may be formed by the taking of pieces till the Kings only remain on the board, then the above comparison does not in the remotest degree suffice to give an adequate idea of the vastness of the possible numbers.

Mr. R. A. Proctor's Lecture Tour.

Subjects:

1. LIFE OF WORLDS
2. THE SUN
3. THE MOON
4. THE UNIVERSE.
5. COMETS AND METEORS
6. THE STAR DEPTHS
7. VOLCANOES.
8. THE GREAT PYRAMID.

Each Lecture is profusely illustrated.

Arrangements are now being made for the delivery of Lectures by Mr. Proctor. Communications respecting terms and vacant dates should be addressed to the Manager of the Tour, Mr. JOHN STUART, Royal Concert Hall, St. Leonards-on-Sea.

Sept. 7, 9, 10, Scarborough; Sept. 8, 11, 15, York; Sept. 14, 16, 21, 22, Harrogate; Sept. 17, 18, Whitby; Sept. 23, 24, 25, Ilkley; Sept. 28, 29, Derby.

Oct. 2, Chester; Oct. 3, 17, Malvern; Oct. 6, 9, 12, 13, Plymouth; Oct. 7, 10, 14, 16, Torquay; Oct. 19, 22, 28, Salisbury; Oct. 21, 26, 29, Southampton; Oct. 23, 27, 30, Winchester. Oct. 31, Marlborough College.

Nov. 2, Chester; Nov. 4, Burnley; Nov. 9, Stafford; Nov. 10, Streatham; Nov. 12, Middlesbrough; Nov. 17, Darwen; Nov. 19, Saltaire; Nov. 25, 28, Bath; Nov. 26, 30, Clifton.

Dec. 2, 5, Bath; Dec. 4, Clifton; Dec. 7, 8, 9, Croydon; Dec. 11, Chester; Dec. 16, 17, 18, 19, Leamington.

Jan. 12, Hull; Jan. 15, Stockton; Jan. 26, Bradford.

Feb. 3, Alexandria; Feb. 5, Chester; Feb. 6, 20, Malvern; Feb. 9, 12, 19, Cheltenham; Feb. 10, Walsall; Feb. 15, Upper Clapton; Feb. 18, 25, London Institution. Feb. 22, Sutton Coldfield.

March 1, 3, 5, Maidstone.

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